THE

AMERICAN

JOURNAL OF PSYCHOLOGY

EDITED BY

G. STANLEY HALL

VOL. V.

WORCESTER, 1893
J. H. ORPHA, Publisher

PRESS OF F. S. BLANCHARD AND COMPANY
WORCESTER

THE AMERICAN

JOURNAL OF PSYCHOLOGY

VOL. V OCTOBER, 1892. No. 1

DISTURBANCE OF THE ATTENTION DURING SIMPLE MENTAL PROCESSES.

BY EDGAR JAMES SWIFT.

The purpose of the present investigation was to test the accuracy of earlier experiments and to throw new light on some aspects of the subject. The experiments were made in the laboratory of Professor Ebbinghaus, in Berlin, beginning in May, 1890, and extending to February, 1891. The laboratory consists of a single room; this made it impossible for the reactor and the one who conducted the experiments to be separated. There was no conversation, however, during the progress of the experiments, except as the reactor said "worthless" when some abnormal conditions made the value of a certain reaction doubtful. The work was carried on late in the afternoon and in the evening, when the surroundings were quiet. A signal always preceded the excitation by about three fourths of a second.

A Hipp chronoscope of the new pattern was used. Its accuracy was tested regularly. The description is given in Wundt's Psychologie.[1] The reactor was George A. Coe, Ph. D., an American, who kindly lent his assistance to my investigations, and to whom I am indebted for many valuable suggestions.

The sound which served as the excitation for the simple muscular reactions was caused by the striking together of two weights. Each weight was connected with the circuit in such a way that when they touched one another the circuit was closed. This sound called for a response with the first finger. The striking together of the same weights gave one of the sounds when a choice between two movements was to be made. The first and second fingers of the right hand were used in reacting. The key-board consisted of five keys connected in the usual way. The sound of the weights was

[1] 3d. ed. p. 275 VII.

responded to with the first finger, by the pressure of which, upon the key, the current was broken and the chronoscope stopped.

The other sound was produced by the dropping of a ball from the arms of a Hipp fall apparatus.[1] The moment the ball reached the board below the electrical current was closed and the chronoscope started. The muscles of the arm and hand were tense, ready for action, and the attention closely concentrated on the movement to be made. As soon as possible after hearing the sound, the reactor would break the current by pressing a key.

The preliminary experiments suggested other questions, which stood in such close relation to the ones at issue that their investigation seemed desirable. It has been thought best to first introduce some of these experiments, which really came later in the series, as in many cases they have a bearing on the others.

For some time I have been convinced that it is a wrong method to take reactions of one or possibly two kinds, on certain days, and finish those of one sort before beginning the others. Reactions whose results are to be placed in immediate comparison with one another should be taken on the same days, and the order of succession of the different kinds should be changed from day to day, so that the disturbing influences of weariness, practice, and other less easily controlled conditions, may, so far as possible, be eliminated or equalized.

In some of the earlier experiments which I made, in which a choice between two movements was necessary, the reactors said that they found themselves at times involuntarily "guessing" which of the two sounds would come next, and they thought that the reaction followed much more quickly sometimes, on account of their correct conjecture. J. V. Kries[2] has already called attention to the fact that a reaction will follow more or less quickly according as a presentation of the movement to be carried out is already in the mind or not. In order that this might be tested, two series of experiments were made, which I will call the "chance" and "non-chance" series. In the "chance" series the succession of the two sounds which served as excitations, and to each of which a particular movement responded, was previously determined by tossing of a copper coin. In the "non-chance" series I followed the dictates of my mind in the succession, but made every possible effort to keep the order uncertain.

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[1] Wundt's Psychologie, 3d ed. II. 275.

[2] Vinteljahrschrift für Wissenschaptliche Phil. 1887 II. 4.

The experiments were performed according to the plan mentioned above. Some of each of the two kinds were taken every day, and those that came first one day would come last the next. In this way it was hoped to equalize many disturbing and uncertain elements.

A high card-board partition separated Dr. Coe and myself. It was thus impossible for him to receive any suggestions from such movements as I might make.

Table I. shows the results of the first set. The purpose was to determine the difference between the "chance" and

TABLE I.

Date.	Reaction[1] in Chance Series.	M. V.[2]	Number of Experiments.	Reaction[1] in Non-chance Series.	M. V.[2]	Number of Experiments.
1890.						
Nov. 17	218	30	23	193	35	38
" 18	230	29	45	198	33	53
" 19	204	29	49	206	21	51
" 20	210	22	49	209	30	61
" 21	205	25	39	216	24	37
" 21	204	21	18	188	19	22
" 22	203	33	27	197	23	28
" 22	198	23	26	193	26	34
" 24	210	18	25	193	26	34
" 25	166	19	31	169	21	34
" 25	185	31	31	169	21	35
" 26	175	20	30	188	25	31
" 27	177	28	31	169	27	37
" 28	187	32	30	167	18	33
" 28	176	26	29	160	17	33
" 29	170	22	30	160	25	34
	195	25		186	24	

[1] The figures denote $\sigma = 0.001$ sec.
[2] M. V. = mean variation.

"non-chance" method. Sixteen averages, gained from a varying number of trials, are given in each series, and in nine of these the "chance" series is markedly longer than the "non-chance." In two cases the "chance" is less than the "non-chance," and in five the two are so nearly equal as to be considered practically the same. In three of these the excess, though slight, is still in favor of the "chance" series. The average difference is 9 σ.

The reactor did not know the order of the two series, but he always "felt" a difference in them. Each day at the close of the evening's work he would guess which had been the chance series, forming his opinion from the feeling of greater ease which was associated with the one or the other series, and almost without exception his judgment was correct.

A doubt, however, suggested itself. Perhaps Dr. Coe's knowledge of the problem to be solved unconsciously influenced his reactions. That this might be tested I asked Mr. Mulfinger, also an American, to react for a few days. Mr. Mulfinger had gained experience in reacting during the previous semester, but knew nothing whatever of the question at issue until all the experiments had been completed. The result of 100 experiments in each series was as follows: reaction-time for the chance series 193σ, with a mean variation of 29σ, and for the non-chance series 168σ, with a mean variation of 23σ. Mr. Mulfinger also felt less difficulty in responding to the "non-chance" series, though he had no knowledge whatever regarding the object of the experiments or the difference between the series.

The inference from these experiments is clear. The mind of the experimenter works according to certain laws of regularity which he cannot escape. This regularity will assert itself in the way in which he chooses between the two stimuli and in time may disclose itself to the other mind, though the latter is conscious of it only through a vague feeling. Had I not tried to make the non-chance order perplexing to the reactor, the difference between the two series might have been still greater, as the mind would then have been free to follow its inclination.

In both of the preceding cases the number of single experiments made at one sitting varied from twenty to sixty. The difference of the two kinds was not found when the experiments were made in short series of 9 to 16, the averages being practically the same. A little time seems to be necessary before the reactor's mind can become master of the law of regularity which the other unconsciously follows.

It seems probable, therefore, from the results of these investigations that all experiments in which a choice is involved

should be made either in a chance order or the number of successive trials should be limited to ten or twelve. Otherwise the law of regularity of the mind will betray itself in the order of succession and the results will contain a constant error. In all of the following experiments in which a choice forms a part, I have followed a chance order, determined as before by the tossing of a coin. This was necessary in investigations of this nature because the stopping of the work at the end of twelve trials would have kept the reactor's mind in a constant state of unnatural excitement. The importance of this will be seen when we reach the experiments that test the influence of different grades of disturbance.

We now turn to the first set of experiments to show the effect of a disturbance on the reaction-time. These preceded the experiments given in Table I., as will be seen from the dates, but as they do not involve a choice the question of chance does not affect them.

It is evident that when a question of a disturbance of the attention is being investigated, great care must be taken in rejecting figures. When the numbers to be discarded are judiciously selected the results can be made to support almost any previously acquired theory. For this reason I have rejected few figures and seldom when the reactor did not himself say immediately after reacting that some unusual disturbance had made the reaction valueless. In the other few cases where a figure was rejected, it varied so much from any of the others as to leave no doubt of its worthlessness.

The arrangement of the apparatus was the same as before. The large card-board partition kept the reaction free from such disturbances as might otherwise have influenced his reactions.

The following set consists of two series, the simple muscular reaction[1] in response to an excitation of the sense of hearing and the same taken while a metronome was ticking one hundred and twenty times each minute. Not only were some of both series taken every day, but the order in which they were taken was also alternated, so that on one day the simple muscular reaction preceded the muscular reaction with the disturbance, while on the next day the order was reversed.

As the result of 100 experiments in each series made under exactly similar conditions, the simple muscular reaction-time was 103σ, with a mean variation of 9σ, whereas the reaction-time, with disturbance of the attention by the ticking of the metronome, was 122σ, with a mean variation of 12σ, the atten-

[1] WUNDT, *Physiologische Psychologie*, II. 265, Leipzig 1887.

tion being given to the movement to be made. The lengthening due to the disturbance is 19σ.

It would seem that the ordinary muscular-reactions are affected by a disturbing sound. Obersteiner's investigations[1] have already supported this view, but his method was hardly satisfactory. The playing of a music box served as his disturbance and the reaction-time under these conditions was learned. That the time required when the attention was undisturbed might also be found, the playing occasionally ceased just before the excitation was given. The sudden stopping of a sound would tend to startle the reactors and the reaction following would hardly be reliable.

Cattell claimed[2] that the reason why some earlier investigators found the reaction time lengthened by a disturbing sound was probably due to the fact that the reactors had not learned to make their reactions automatic. It can hardly be considered as yet fully settled that the muscular reactions are strictly automatic, if by this he means purely brain reflex. This has lately been made doubtful by the investigations of Dr. Götz Martius[3]. Dr. Coe, whose reactions are given in Table IV., had reacted from the beginning according to the so-called muscular method, and his instructions during these experiments were to keep his muscles innervated and his attention strictly on the movement to be carried out. No attention whatever was to be given the disturbance and the reaction was to follow the excitation as quickly as possible. Wundt in the third edition of his *Physiologische Psychologie*, II. 290, supports Cattell's opinion.

In view of these results I am unable to agree in the opinion that muscular reactions are not lengthened by disturbing sounds. The separate averages are fairly regular and in every case the muscular reactions taken during the disturbances are longer than the others. The final average, gained in each case from one hundred trials, gives a difference of 18σ between the simple muscular reaction without disturbance and the same kind of reaction taken while the attention was disturbed.

In connection with these results it may be interesting to notice some others that differ only in one respect. In the experiments which we have just considered the attention was directed as closely as possible to the movement. In the next set it was given to the ticking of the metronome.

Some of the muscular reactions without a disturbance were also taken each day in connection with the others in order to gain a reliable basis of comparison.

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[1]Brain, 1879, I. 447.
[2]Philosoph. Studien III. 329.
[3]Philosoph. Studien VI. 167.

With the attention fixed upon the disturbing sound the result of 50 experiments without disturbance and 100 with disturbance, made on three successive days, was 110σ mean variation, 7σ for the simple muscular reaction and 158σ mean variation, 14σ for the reaction with disturbance. The difference due to the disturbance is 48σ.

Comparing these figures with those obtained when the attention was directed toward the reacting finger, we find that the difference between the time of reaction during a disturbance to which the reactor gave no attention and the time required when the attention was directed to the disturbing sound is 36σ.

Let us now pass to the experiments in which a choice between two movements was necessary. The sounds to which the movements responded were caused in the manner already explained. The first finger of the right hand reacted to the sound of the two weights as they were struck together and the second finger to that of the falling ball.

The disturbing element was the metronome ticking one hundred and twenty times each minute as before.

The results were as follows: Reaction with choice 179σ (mean variation 21σ; 100 experiments); reaction with choice, but with a disturbance, the attention being directed as closely as possible to the association of stimulus and movement, 197σ (mean variation 25σ; 100 experiments); reaction with choice, but with a disturbance, the attention being directed to the disturbance, 265σ (mean variation 27σ; 100 experiments). The lengthening due to the disturbance is 26σ in the first case and 86σ in the second.

If these results be compared with the simple reactions given above, it will be noticed that, unless a choice is involved, a disturbance has much the same effect as in the simple reactions, provided the attention is directed in the one case to the correspondence of movement and sound and in the other to the movement itself. The increase is 19σ in each case.

When the attention is given to the disturbing sound, however, the lengthening of the time is much more marked in the choice series. This is not surprising, because the attention, after turning from the disturbances to the reaction, would lose more time in associating the given excitation with the corresponding movement than in making a single movement already determined. The lengthening is 48σ for the simple reaction as contrasted with 86σ for the choice-reaction.

It is interesting to observe that while the mean variation increases somewhat under the influence of a disturbance, nevertheless the increase is not very great.

During the progress of these experiments the possibility of

distinguishing a little more closely between disturbances suggested itself. The mind seems at times more in unison with certain disturbing sounds than with others. Is it not possible that this will show itself in the reactions? If this is true, clearly we will have no right in the future to speak simply of a "disturbance of the attention," but must first measure its effect in comparison with other disturbances of the same kind, but differing in intensity. In order that this might be tested we arranged a set of experiments with a graded disturbance.

The excitation consisted as before of the two sounds and the response was given with the first and second fingers of the right hand. Five series were taken and the metronome ticked 40, 80, 120, 160 and 200 times each minute in the respective series. Each contained one hundred trials; the only exception is the series with 120 beats, which contained ninety trials. As before, some of each were taken every day and the order was always changed.

The results are shown in the following table:

TABLE II.

Number of ticks.	40	M V.	80	M V.	120	M V.	160	M V.	200	M V.
Dec. 11	201	14	218	15	185	20	202	23	200	19
" "	213	20	198	19			204	13	217	15
" 12	223	33	200	25	185	19	221	18	211	14
" "	210	13	215	32	188	18	215	16	232	25
" "									208	16
" 13	206	22	206	21	202	19	197	20	190	16
" "	203	20	227	14	202	22	194	15	197	17
" 15	199	13	204	11	188	13	197	18	198	14
" "	208	17	211	25	185	12	203	31	200	19
" 16	203	13	210	8	184	28	224	21	205	17
" "	198	13	213	17	204	19	221	15		
Average	206	18	210	19	191	18	208	19	206	17

The experiments were grouped in sets of 10 each in consecutive order; each number in the table thus represents the

average of 10 experiments, the lower line giving the average for the whole hundred.

In almost every case the partial average of the experiments made while the metronome was giving one hundred and twenty sounds each minute is lower than any of the others. In only a few cases is it higher. From the point with one hundred and twenty ticks the reactions are at first longer and then they slightly decrease. It would seem that there is a *point of least disturbance* which may be approximately found by means of the reactions. This point of least disturbance probably differs with different persons. Dr. Coe was not told at what point the metronome was set, though this would partially betray itself in the ticking. The difference found cannot be the result of practice or weariness, as some of each series were taken every day and the order of succession was never the same.

We now turn to the investigations with reference to the sense of sight.

These experiments were carried on during the evenings when it was quite dark. No light was visible except that which at the proper time served as a disturbance. In order that the chronoscope might be read without lighting the room, a small dim lantern was so arranged that a dark paper funnel connected it with the chronoscope.

The light which served as the stimulus was a Geisler tube, through which a current of electricity could be passed. A card-board about three feet high and four feet long was placed upright directly in front of the reactor and at a short distance from him. A large mirror was attached to the front of the card-board and a hole three inches long and one half of an inch wide in the center of the mirror corresponded to a similar one in the card-board. Through this hole the light of the Geisler tube, which was immediately behind the board, could be seen the moment the current passed. When the simple muscular reactions were taken the mirror was not used. For the reactions in which a disturbance was desired the mirror was attached and a tallow candle of the ordinary intensity was placed in such position behind the reactor as to be reflected in the mirror. The hole in the mirror cut the image of the flame so that it was seen above and below the opening.

The first set of experiments consisted of three series: first, the simple muscular reactions in response to a light stimulus; second, the same reactions taken while the reflected light of the candle was flickering across the opening in the mirror and card-board, through which in a moment the light of the Geisler tube would be seen. In the third series an attempt was made to still further disturb the attention by the image of a revolv-

ing card. This card was about ten inches long and six wide and was covered with a few plain figures. It was placed behind the reactor so as to be reflected in the mirror around the opening through which the light of the Geisler tube was expected. A rotating apparatus caused the card to slowly revolve. The candle was used to illuminate the disk. In all three cases glass in front of the Geisler tube colored the light red.

The results are as follows: Simple reaction, 140 σ (mean variation, 11 σ; 40 experiments); simple reaction with disturbance by the candle, 148 σ (mean variation, 10 σ; 50 experiments); simple reaction with disturbance by the revolving card, 139 σ (mean variation, 12 σ; 70 experiments).

The simple muscular reactions of 140 σ is a little less than the results gained by Cattell.[1] The difference is readily understood when we remember that Cattell carried on his investigations during the day, and the sunlight reflected from a white surface would probably require more time to come into the consciousness of the reactor than a red light in a darkened room.

A rather surprising fact is that the simple candle flame flickering across the opening in the mirror, was a source of greater disturbance than the revolving card. The reason of this, however, is not difficult to find. The card, though covered with figures, was too monotonous to serve as a disturbance. This may explain the fact that some investigators have found the reactions so little influenced by a disturbance of the attention. The candle distracted the attention somewhat more, because the reactor looked directly at the flickering flame as it fell across the opening through which he awaited the exciting light. In the other series the eyes were directed rather to the image of the card as it slowly turned. Anything so monotonous will hardly influence the reactions, and still less when the reactor has been told to hold his attention closely to the movement to be made.

The three series differ so little that we can hardly say the reactions were affected, especially when we observe that in the case of the revolving card they are, in fact, less than the simple musculary. The mean variation of the three series is about the same.

From these experiments it was evident that some more positive disturbance must be found.

To accomplish this, I replaced the revolving card with another which was of a circular form, twelve inches in diameter. Around the edge of this disk nine holes were cut.

[1] Philosoph. Studien, III. 324.

These holes were one and two eighths of an inch long, and sloped from seven eighths of an inch at the top to one half of an inch at the bottom. This disk was attached to the rotating apparatus, and the lighted candle was placed behind in such a position that the flame shining through the holes in the disk was reflected in the mirror. The rotating apparatus was thus caused to revolve so that five hundred and forty flashes of light fell upon the eye each minute.

The results were as follows: Simple muscular reaction to light, 143 σ (mean variation, 10 σ; 100 experiments); simple muscular reaction with a disturbance caused by 540 flashes per minute, 171 σ (mean variation, 15 σ; 100 experiments). The lengthening due to the disturbance of the attention is thus 28 σ.

The reaction time for a sound sensation has been found above to be 102 σ and 110 σ. The time needed to respond to a sight excitation is thus seen to be about 30 σ to 40 σ longer.

While we were engaged in these experiments the thought suggested itself of arranging a series of disturbances for the sight which could be compared with those already used for the sense of hearing. It would then be possible to learn through which of these two senses the attention is more easily disturbed. This has before been impossible because there has been no unit of comparison for the disturbances.

The problem was readily solved by means of revolving disks similar to the one just used. Three disks were prepared. The first had only one hole, which was of the same size and shape as those already described. The second contained two holes, one at each end of a diameter, and the third had one at each of the three corners of an isosceles triangle inscribed within the circular disk. The rotating apparatus was so regulated that it revolved sixty times a minute. It was thus possible with the three disks to give 60, 120 and 180 flashes of light each minute.

The results were: simple muscular reaction in response to a visual stimulus, the attention being disturbed by 60 flashes per minute, 195 σ (mean variation, 14 σ; 120 experiments); with the attention disturbed by 120 flashes, 197 σ (mean variation, 13 σ; 120 experiments); with the attention disturbed by 180 flashes, 190 σ (mean variation, 16 σ; 120 experiments).

The time required by Dr. Coe to react muscularily in response to a light excitation was 143 σ, as seen above. We now find the time increases to 197 σ, with his attention disturbed by 120 flashes of light each minute. We have already learned that the time of reaction for the sense of hearing was 102 σ. In the experiments made on the same day, when the

attention was disturbed by a metronome ticking 120 times a minute, it rose to 122 σ. The lengthening of the time on account of the disturbance was thus 53 σ for the sense of sight, and 19 σ for that of hearing. It is thus seen that when the disturbance and excitation affect the same sense, a distraction of the attention through the sense of sight has more influence upon the reaction time than the same grade of a disturbance through the sense of hearing.

When the effects of the various grades of disturbance are compared, it will be observed that 540 flashes of light a minute are less of a disturbance than 60, 120, or 180 flashes. The probable reason is that 540 flashes acted as a steady accompaniment on account of the rapidity of the flashes. That which is constant tends to be monotonous, and this monotony may have deprived the disturbance of a part of its effectiveness.

Before discussing the experiments undertaken to determine the effect of a disturbance of the attention when a choice was to be made in answer to one of two sight sensations, it will be necessary to describe the apparatus which was constructed for this purpose. The question then was, how to prepare an apparatus by which two different excitations could be quickly and noiselessly produced and the order changed without altering the psychical condition of the reactor. A long lever was fastened to an upright support by means of a pivot, and it was arranged so as to work noiselessly. A light frame, containing red glass above and olive colored glass below, was attached to one end of the lever, which was so placed that this frame was directly behind the aperture in the mirror which we have already described. Through this opening in the mirror the exciting light of the Geisler tube would be seen as the current passed. The other end of the lever extended to the experimenter at the chronoscope, who could raise or lower the handle at pleasure and thus bring the end or olive glass in front of the tube. The change could be made noiselessly in a moment. When the disturbance was desired the mirror was attached, as before, to the front of the card-board, and the disturbing flashes of light were seen reflected around the hole behind which was the Geisler tube. A high partition separated the reactor and the experimenter.

The choice was made in response to the excitations of the colors red and olive. The disk which gave 180 flashes of light each minute was used as the disturbing element. This, it will be remembered, was the "point of least disturbance" for the sense of sight. The final results consist of eleven averages, each of which was gained from ten trials.

The results of the first set of experiments are: Time of

reaction with a choice to be made between two movements in response to one of two different excitations of the sense of sight, 258 σ (mean variation, 23 σ; 110 experiments); time of reaction of the same kind, but with a disturbance of the attention caused by an intermittent light of 180 flashes per minute, 273 σ (mean variation, 25 σ; 110 experiments). This gives a lengthening of 15 σ as the effect of the disturbance.

The influence of the disturbance as shown in the reactions is thus less than for the sense of hearing under the same conditions. When choice was made in response to one of two sounds, the time was 177σ and 197σ, which leaves a difference of 20σ.

We have already noticed that in the simple muscular reactions when the disturbance and excitation are of the same kind, a disturbance through the sense of sight is more effective than through that of hearing. We now find that when the reactions include a choice the mind is less disturbed in discriminating between sight excitations than between those of hearing. The difference, though slight in itself, is so far the reverse of that which we found characteristic of the simple muscular reactions as to deserve attention.

The psychical processes are the same, whether we respond to a sound or a sight excitation and that which makes up an act of choice does not differ with the sense organ excited. The reason why the muscular reactions in response to a light excitation are more influenced by a disturbance through the same sense than those of sound, while in choice the light reactions are less affected, must be sought then in conditions which hasten or retard these processes rather than in the processes themselves. A large part of our knowledge is acquired through the sense of sight. We are constantly called upon to decide quickly between things that we see, seldom between what we hear. We thus gain a facility in discriminating between objects of sight and in acting according to our dicision. This intimate association of sight and movement in choice leads to an ease and rapidity of action which cannot be equaled when the excitation comes to us through another sense.

We are also more familiar with disturbances in our discrimination between objects of sight. In the street many things crowd themselves upon our sight and in the midst of this confusion we are daily called upon to act. It is then not surprising that our more complicated mental processes, when stimulated to action through the sense of sight, are less impeded by disturbances than when the excitation comes to us through another sense. The time of choice in reply to one of two sounds was found to be 176σ, that in response to one of two sights is 258σ.

We have now reached the experiments which may be called the "cross sets." In these the excitation and disturbance affect different senses.

In these experiments the excitation was the usual sound of the two weights. The disturbance was the 60, 120 and 180 flashes of light per minute.

The results were: time of simple muscular reaction in response to an auditory stimulus, 123σ (mean variation 8σ; 80 experiments); muscular reaction to an auditory stimulus, but with a disturbance of the attention produced by 60 flashes of light per minute, 160σ (mean variation 13σ; 100 experiments); same with 120 flashes, 141σ (mean variation 9σ; 100 experiments); same with 180 flashes, 148σ (mean variation 11σ; 100 experiments).

According to my custom I took a series of the simple muscular reactions each day in connection with those accompanied by the disturbance. The result sustains the view that at least a few of the earlier and simpler reactions should be repeated when the more complicated ones are reached, in order that they may serve as a test of the condition of the reactor. The simple muscular reactions average 123σ; the average as given above is 102σ. In both tables the mean variation is small. The reason for this difference in the results is that the shorter time was gained at the end of a long practice when Dr. Coe had reached his maximum rapidity. The others were taken after a period of over two months, during which time experiments of a different kind were being made.

Cattell[1] found that a facility in reacting once gained was not lessened by lack of practice; v. Kries and Auerbach[2] held the same view regarding the simple reactions, but maintained that it is not equally true of the more complicated. I am o the opinion that even in the simple reaction, lack of practice will manifest itself in a lengthening of the reaction time, and that other physical and mental conditons can so far affect the reactor as to make it necessary when accuracy is desired to repeat from time to time the earlier and less complicated tests.

In the results last given we find again that 120 intermittent disturbances each minute is the "point of least disturbance." It will be remembered that this fact has characterized all the results thus far in which the excitation was a sound.

Earlier investigation[4] has led to the belief that the reactions

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[1]Philosoph. Studien III. 462-489.

[2]Dubois-Reymond's Archiv 1877, 361.

[3]Dubois-Reymond's Archiv 1877, 362.

[4]WUNDT, Physiologische Psychologie II. 293, Leipzig 1887.

are more influenced through a disturbance of the attention if the excitation and disturbance affect different senses than when both are of the same nature. These experiments do not sustain this view. When the metronome vibrated 120 times each minute, the reaction time for a sound excitation was 19σ longer than without the disturbance. The time required to respond to a light excitation while the attention was disturbed by an intermittent light giving 120 flashes each minute was 53σ longer than when the disturbance was lacking. In both of these cases the disturbance and excitation appealed to the same sense organ. On the other hand we find that when the excitation is a sound and the disturbance a light flashing 120 times a minute, the reaction time is lengthened by only 17σ. Even if we take the point of greatest disturbance, 60 flashes, the average is only 37σ longer than the simple reactions, and this is still much below the 53σ already found to be the difference in time between the simple light reactions and those made under the influence of a disturbance through the same sense-organ.

An analogous set of experiments was now made in which stimulus was visual and the disturbance auditory. In these experiments the excitation was the flash of the Geisler tube seen through the red glass and the disturbance was the ticking of the metronome.

The results were: time of simple muscular reaction in response to a visual stimulus 159σ, (mean variation 10; 100 experiments); same with a disturbance of the attention produced by 60 beats per minute of the metronome 176σ, (mean variation 10; 100 experiments); same with 120 beats 190σ, (mean variation 12σ; 100 experiments); same with 180 beats 174σ (mean variation 11; 100 experiments).

If we take the point of greatest disturbance, which was 120 vibrations of the metronome, the time of reaction is 30σ longer than without a disturbance. This again is much less than the corresponding increase when a light served both as excitation and disturbance.

This lengthening of the time when both the excitation and disturbance affect the sense of sight, cannot have been caused by the disturbing light weakening the excitation, because the exciting light was of sufficient intensity to be clearly seen, even when its appearance and that of the intermittent light were simultaneous. Were this the occasion of the difference it would betray itself in an unusually large mean variation. At times the exciting light would be seen more distinctly and the reaction would follow more promptly. That this is not the case a glance at Table XI. will show. The mean variations are very small, considering that the reactions were made under

the influence of a disturbance. They scarcely differ from those for the sense of hearing as given in Table VI.

Besides, if the more noticeable effect of the light disturbance on the sight reactions is the result of a weakening of the intensity of the excitation, why does not the same peculiarity manifest itself in the choice-reactions for the sense of sight? The same weakening of the intensity would occur in the latter as in the former case. We have found, however, from the results given on page 13 that the time of choice in answer to a sight sensation is 258σ. During the disturbance the reactions average 273σ, an increase of only 15σ, and yet both excitation and disturbance were the same lights that we used in the simple reactions. Though we have found (page 11) that 180 flashes was not the point of greatest disturbance, it would nevertheless make more light and so tend more to weaken the excitation than the 120 flashes, which actually proved a greater disturbance. It is evident, then, that the cause of the difference cannot be found in weakening the intensity of the excitation by the disturbance.

The fact seems to be that in simple muscular reactions an intermittent light is more of a disturbance to the attention than a sound. This has been especially noticeable in the earlier experiments (page 12). If we compare once more the results on pages 14 and 15 we find that both 60 and 180 flashes of light in the "cross sets" proved a source of greater disturbance than the same number of vibrations of the metronome. Here the question of the disturbance weakening the intensity of the excitation cannot even be raised as they affect different senses. Under 120 the sound has the greater influence on the reactions. This average, however, is the largest of the three for the influence of auditory disturbances on visual reactions, and if we place it in comparison with the largest for the influence of visual disturbances on auditory reactions, we find still the light proves the greater disturbance.

It has already been shown that a disturbance of the attention does not have the same effect upon reactions which include a choice as upon the simple muscular reactions. In the latter case we have seen that a disturbance of the attention through sight is more effective in lengthening the reaction time than when the disturbance comes through the sense of hearing. On the other hand, whenever the reaction follows a sight sensation, the time of choice is less affected by disturbances of the attention than if the excitation is a sound. This view is strengthened by two series of experiments on reaction with choice, but with disturbance of the attention.

In the first series of the reactions with choice the excitation consisted of two sounds, produced in the manner described un-

der choice in the earlier experiments. The disks served as the disturbance; they gave 60, 120 and 180 flashes of light each minute.

The results were: time of muscular reaction with choice to one of two auditory stimuli, 230 σ (mean variation, 20 σ; 100 experiments); the same with a disturbance of the attention caused by 60 flashes of light per minute, 267 σ (mean variation, 21 σ; 100 experiments); with 120 flashes, 243 σ (mean variation, 19 σ; 100 experiments); with 180 flashes, 255 σ (mean variation, 18 σ; 100 experiments). This gives a lengthening of 37 σ, 13 σ and 25 σ respectively.

The next series consisted of reactions to sensations of sight, in which a choice between two movements was involved. The attention was disturbed by beats of a metronome.

The results were: time of muscular reaction to visual stimuli with choice, 266 σ (mean variation, 18 σ; 100 experiments); the same with an auditory disturbance of 60 metronome beats per minute, 278 σ (mean variation, 20 σ; 100 experiments); with 120 beats, 267 σ (mean variation, 19 σ; 100 experiments); with 180 beats, 277 σ (mean variation, 17 σ; 100 experiments). The reaction time with choice is thus lengthened to the extent of only 12 σ, 1 σ and 11 σ by the disturbance. Here, as in the previous experiments on sight-reaction with choice disturbed by flashes of light, the time of choice in response to sight excitation is found but little influenced by disturbances. This has been found characteristic of reactions in which a choice is involved. The importance and probable explanation of the fact I have already discussed.

We now turn to several sets of experiments which preceded those which I have thus far given; it has been thought best, however, to introduce them last. In the choice sets the "non-chance" method was used, and it was while engaged in these investigations that the difference between the two methods was noticed.

The purpose of these experiments was to find how the simple "muscular" and the "choice" reactions would vary while the reactor's attention was directed to certain kinds of work. Three tasks were given; 1, repeating a poem already committed to memory; 2, reading an English book; and 3, reading Kant's Kritik der einen Vernunft. The time for the simple muscular and choice reactions under these conditions was learned. The instructions were to fix the attention as closely as possible on the work assigned. I willingly admit that it is impossible to determine how far the instructions are observed in such a case. There are no means by which the amount of attention given to the reaction or to the task can be measured. I am convinced, however, that the direc-

tions were conscientiously followed, partly from my confidence in the reactors and their interest in the investigations, and partly from the fact that at times the reactor failed to respond to the excitation and then excused himself by saying that he was so deeply absorbed in his reading that he did not hear the sound. This occurred while the English book was being read.

The reactors were Prof. A. L. Gillett, of the Hartford Theological Seminary, and Mr. George Mulfinger, also an American. Both of these gentlemen offered valuable suggestions.

The excitation in the four sets was a sound caused in the same way as in the preceding experiments.

The results of the experiments with Mr. Mulfinger were as follows: simple muscular reaction to an auditory stimulus, 102 σ (mean variation, 7 σ; 200 experiments); simple muscular reaction while repeating a poem, 183 σ (mean variation, 23 σ; 200 experiments); simple muscular reaction while reading English, 196 σ (mean variation, 20 σ; 200 experiments); simple muscular reaction while reading Kant, 210 σ (mean variation, 27 σ; 200 experiments). The three distractions of the attention caused a lengthening of 81 σ, 94 σ and 108 σ respectively. When Mr. Mulfinger was required to react after a choice, the results were: reaction with choice, 208 σ (mean variation, 24 σ; 200 experiments); reaction with choice while repeating a poem, 212 σ (mean variation, 24 σ; 200 experiments); reaction with choice while reading English, 216 σ (mean variation, 22 σ; 200 experiments); reaction with choice while reading Kant, 217 σ (mean variation, 27 σ; 200 experiments). The lengthening due to the disturbances was thus 4 σ, 8 σ and 9 σ respectively.

With Prof. Gillett as reactor the results were: simple muscular reaction, 107 σ (mean variation, 6 σ; 250 experiments); simple muscular reaction while repeating a poem, 170 σ (mean variation, 16 σ; 250 experiments); while reading English, 179 σ (mean variation, 17 σ; 250 experiments); while reading Kant, 197 σ (mean variation, 17 σ; 250 experiments). This gives an increase of 63 σ, 72 σ and 90 σ for the three disturbances.

The time of reaction with choice was 235 σ (mean variation, 28 σ; 250 experiments), which increased to 270 σ (mean variation, 31 σ; 250 experiments) while a poem was repeated; to 317 σ (mean variation, 35 σ; 250 experiments) while English was read, and to 357 σ (mean variation, 35 σ; 250 experiments) while Kant was read. We find here a lengthening of 35 σ, 82 σ and 122 σ in the three cases respectively.

The difference here is not so marked. In the first case the increase is in favor of the muscular, and in the other two

with the "choice" reactions. It is clear, however, that the complete separation of the two kinds of reactions by which the muscular are placed beyond the influence of a disturbance of the attention, and regarded as purely practiced brain reflex, while the others only come within the reach of psychical disturbances, cannot be regarded as established. I am led to this conclusion, not alone by these latter experiments, but also by all those preceding. A comparison of results shows a different of only 1σ in favor of the "choice" reactions on account of the disturbance. This was for a sound excitation, and is practically no difference. If we examine the results, we find a difference of 12σ in favor of the muscular reactions because of the disturbance, and yet this 171σ is the least of the effective disturbances that we have found for the sense of sight.

PSEUDO-CHROMESTHESIA, OR THE ASSOCIATION OF COLORS WITH WORDS, LETTERS AND SOUNDS.

By WILLIAM O. KROHN, PH. D., Fellow in Clark University.

The first problem of psychology is the study of sensation. A wealth of material comes to the investigator as the deliverance of the various special senses when the several end-organs are appropriately stimulated. But the *pseudo* sensations constitute the subject matter of psychology just as much as those arising from "bona fide" stimuli. Indeed, much light is thrown upon the problems of psychology by following out this line of study and investigation. Some of the most useful as well as the most interesting psychological material comes to us in the form of *pseudo* sensations. Of all the interesting phenomena which fall under this head, the pseudo sensations of sight are the most numerous. The present paper deals with one form of pseudo photesthesia, to wit—that large class of phenomena in which colors are called up in the mind of the subject when certain letters or words are spoken, or seen in print or writing. The term "color audition," so commonly used, does not cover all the cases, for there are instances, as the context will show, in which individuals have these pseudo sensations of color when they *see* words, but *not* when they hear them enunciated. The term we choose as a sort of label for this interesting class of phenomena is that of "pseudo-chromesthesia." Pseudo-chromesthesia is that peculiar faculty of association of the sensorial perceptions, by means of which any primary sensation, or even a purely psychical process can evoke, in the case of certain persons, a false visual sensation of color, constant in the case of the same stimulus with the same person. The phenomena may be of optical origin, i. e., the efficient cause may come through the eye (or the memory image of a visual sensation) of the graphical forms of a letter, a number, a geometrical figure, such as a circle, triangle or square. They are of acoustic origin when the efficient cause passes through the ear or is a memory image associated

with that organ. Thus every noise, every sound perceived objectively, or evoked mentally can arouse these pseudo sensations of color. This is especially noticeable in the case of the human voice, as in public speaking or reading aloud.

A.—Historical and Descriptive.

The great poetic genius, Goethe, was one of the very first to make reference to this subject, which he does in his Theory of Colors (1810). In this connection he refers to the little writing by Hoffman, which was published in 1786, in which a case is cited of a Swiss magistrate and painter who seemed endowed with the power of giving color to sounds. The sounds of the various musical instruments evoked strong color impressions, which were especially vivid in the case of the high notes. Thus the notes of the violoncello seemed to him, indigo blue; clarionet, yellow; trumpet, bright red; flute, dark red; violin, very bright blue; hautboy, rose color; and the flageolet, violet.

The first case of pseudo-chromesthesia to find a place in the medical journals, and the first, as well, to receive a minute description, is that detailed by Dr. George Sachs—a case which occupied a considerable portion of his inaugural dissertation at Erlangen in 1812, written in Latin, and afterwards translated (1824) into German by Schlegel. The person to whose peculiarity he gives so much space was an Albino, and colored the following:

1. Vowels.
2. Consonants.
3. Musical notes.
4. Sounds of instruments.
5. Figures.
6. Names of cities.
7. Days of the week.
8. Dates.
9. Epochs of history.
10. Phases of human life.

His pseudo-color sensations were of three kinds with reference to their origin — optical, acoustic, and purely psychical.

The vowels appeared as follows:

a = vermilion red.
e = rose.
i = white.
o = orange.
u (oo) = black.
ü = white.

Of the consonants:

c = ash.
d = yellow.
f = opaque white.
h = blue grey.
k = dark green.
m = white.
n = white.
s = navy blue.
w — brown.

Musical notes generally had the same color as the letters which would indicate these notes.

In the case of the numbers:

1 = indistinct white.	6 = indigo.
2 = uncertain.	7 = bluish white.
3 = ash color.	8 = brown.
4 = red.	9 = dark green.
5 = yellow.	0 = pale uncertain yellow.

It is an interesting observation that in groups of numbers of several figures, each group takes the color of the *last* figure, but the tints remained rather distinct without entirely merging into a single color, the figure of a superior order dominating in general. Zero does not change the color of the figures to which it is joined, but does change the general appearance, e. g., 10, 11, 100, 110 and 111 all are white, but they differ in brightness and clearness. This subject had local colors for each of the cities which did not always correspond with the colors he gave to the isolated letters, which, when grouped together, constituted the name of the city. Likewise as to the days of the week. Sunday was white, sometimes slight yellow; Monday, a cloudy white; Tuesday, indistinct tint; Wednesday, yellow; Thursday, green; Friday, obscure white; Saturday, a bluish ash color. It is to be noticed that the colors of this man were not equally distinct or intense, and that he did not perceive in his pseudo photesthesia all the colors of the spectrum.

Some of the early observations along this line were those of Gautier in 1843. He contributed nothing new, except in recording the fact that he could produce these pseudo sensations of color artificially, in particular by the use of hasheesh.

In 1848 Dr. Cornaz sought to stir up investigations and researches into the matter by publishing a little brochure on the hyperesthesia of colors. His efforts were not entirely fruitless, for very soon after, Dr. Wartmann gave out an abridged description of two cases. In 1855, Joachim Raff, the composer, declared that the sounds of instruments produced color impressions of various kinds. Thus the sound of a flute produced the sensation of intense azure blue; of the hautboy, yellow; cornet, green; trumpet, scarlet; the French horn, purple; and the flageolet, gray. The clearest and most distinct shades were those evoked by the high notes. Also Ehlert in one of his musical letters from Berlin describing one of the symphonies of Schubert, says: "The air in the scherzo is a sunny warmth, and a green so tender, that it seems to me that I breathe the odor of young fir trees."

Chabalier published some interesting cases in 1860, but

nothing at all new was contained in his little book, and in the same year Vauthier published a little account of his own experiences, in which a certain sound awakened not only a color impression but also an excruciating toothache.

But Perroud, in 1863, was the first man to recognize the physiological character of these phenomena, and also the first to attempt an explanation. The case he records is that of himself, and is of especial interest because it was a case in which the impressions were aroused by *optical* and not by auditory stimulus. His experience with these phenomena did not begin until he was about fifteen years of age, when he began to see certain letters, especially vowels, intimately bound with colors.

With Perroud :

a = orange yellow.	o = canary yellow.
e = bluish or pearl gray.	u (oo) = sombre brown.
i = carmine.	

Diphthongs give the sensation of *two* distinct colors, but this is not the case with compound vowels. These form a single tint, a trifle more intense than that produced by a single vowel. V appears greenish, while all of the other consonants partake of the color of the neighboring vowel (irradiation). Figures are also united by him to ideas of color, but less distinct—1, carmine (same as i); 2 and 3, gray; 4, sombre brown; 5, indistinct; 6, green; 7, carmine; 8, canary yellow [might not 8 be two o's? ($\substack{o\\o}$)]. It is a point of great interest that with this person the colors become more intense the further one reads, and the more tired he becomes, as well as the more his attention is concentrated.

Chabalier again took up the matter, and in a more thoroughgoing way, in 1864. His explanation was also physiological like that of Perroud. With him a is a deep black; e, gray; i, red; o, white; and u (oo), sea-green. He observes that, as *printed*, these vowels appear uncolored, and perfectly black, but he could have no mental concept of them without arousing the colors as given above. He describes the consonants as "dead, inanimate and entirely secondary." With him proper names had very vivid colors according to the grouping of the vowels. The color much more vivid than the name. Would often forget names, but never the color of the name. With numbers he noticed that 2 and all numbers terminating in 2 seemed white merging into gray; 5 and all terminating in 5, a vermilion red. The days of the week also had colors as well as the months of the year. Thus, Juin, very red; Juillet, light red; Août, sea-green; and all the "ber" months were an earthy gray

(colored by e). In 1865 Verga observed some new cases and revised Chabalier, and in 1871 Kaiser also observed some new cases, which he treated in his compendium on optical psychology.

Perhaps there is no contribution of more interest than that of Nussbaumer, who, when a student at Vienna, at the age of 23, published his observations and those of his brother. They both possessed the finest discriminative ability for sounds from a very early age. From the age of 4 or 5 years they both experienced the phenomena of color audition. In one of their games they used three spoons with strings attached to make bells, and each tried to excel the other in making sounds, claiming different ones produced this or that color, the discussion always ending in a fight. With these two brothers the perception of color seemed purely subjective. Of the letters they colored the vowels only and not the consonants. With F. Nussbaumer, of the piano sounds, *re* is chestnut brown; *fa* is brown with gray lines; mi_2 is sombre brass color at the beginning of the sound and blue at the expiration; *la* is chamois skin yellow; la_3 is a clear orange yellow; fa_4 is a transparent lemon; *sol* is yellow changing into blue; do_3, white at beginning and then changes into transparent light blue. The sound of a rolling carriage is a gray mixed with yellow; that made by a saw is green. The voices of men, if effeminate and sharp, are like the color of a faded leaf in autumn; harsh voices are a brownish gray. The highest tones of a trumpet are a golden yellow and diaphanous; a little table bell, yellow at first and bluish toward the end.

These sensations of color with John Nussbaumer were also of acoustic origin. The high sound of a trumpet gave him the impression of lightning. Other sounds called forth simple tints of color. In 1876 Prof. J. Nuel made a résumé of the facts related by Nussbaumer and set himself to explain them. Of this attempt we shall speak later.

Of all the works on this class of sensations, none is more familiar or more frequently quoted than that of Bleuler and Lehmann. We will cite but one of his cases in detail, and select the one we do because it is a clear case of color *hearing*. The subject (the 77th) maintains that the form of a letter has but little to do in evoking color impressions, but the least change in the sound or quality of the tone greatly changes the shade of color.

Words learned before he knew his letters, e. g., "millionen," have a single color, while others take their color from the component letters. Of the languages, French is sombre brown; German is green; English, light brown; Italian, bluish; ancient Greek, yellow; and Hebrew a som-

bre tint. Up to this point it is, as stated above, a case of color audition; but when we come to the figures, we find that the various digits evoke color sensations entirely independent of sound, and it thus becomes a case of pseudo-photesthesia. In the case of geometrical figures, a succession of acute angles, ,gives a light tint; a series of obtuse angles, , gives a little darker; and a continuous wavy line, , gives a still darker shade. A circle gives a bright yellow tint, while a triangle gives a clear, bright, silvery image, and the rectangle also a very light color.

The low sounds with this subject were all black, passing as they rise in the scale into a reddish brown and brownish yellow in turn. The middle sounds are yellow and the most elevated are pure white. Surely the "chromatic scale" should possess no terrors for such a man. The noise of respiration is gray. A crackling sound is made up of white points; a tremulous sound is a light bluish gray.

With this subject the color associations transcend the ordinary limits, for he has them awakened by senses other than those of vision and audition. Thus, with the sense of smell there is no odor without a color sensation. A cold in the head changes the color of these odors. The odor of vanilla is a light lilac; of the rose is rose color; odor of ammonia is whitish; of vinegar, red; of cologne at a distance, reddish, but under the nose it becomes a transparent gray. In the realm of taste — a sweet taste is red, a bitter taste is a dark brown; with vanilla the taste evokes the same color as the odor, showing that the so-called sensations of taste are largely those of smell. The painful sensations color themselves according to their intensity: thus, violent pain is white; heavy headache is black; an intermittent sharp pain is made up of white points, the same as a crackling noise; a pinch gives a yellow color. The days and months are also colored, but independent of sounds.

In 1882, Pedrono, the well-known rhetorician of Nantes, published a case of no little interest. He seems to have colored sounds rather than words or letters. These color impressions he describes as sudden and spontaneous. *The sounds are translated into color before he can stop to think whether the voice is high or low.* He externalizes an image hovering round about and above the person singing or producing the noise. Vowels when standing alone give *no* chromatic sensations, only when uttered, and then they take their color from the tone of voice. In general, low sounds are sombre and high sounds are bright. Every noise, whether a harmony, discord, crash or clang, produces a

chromatic sensation. Of voices, yellow voices are the most agreeable, and fortunately the most frequently met by him. The same melody when played on different instruments gives different color impressions. Upon a harmonium or tenor saxophone it is yellow; clarionet, red; and piano, blue. Whether the person is seen or not the color is there; whether the eye is opened or closed makes no difference.

In his little work published also in 1882, G. Mayerhausen tells of a lady of 25, a wife of a physician, who had some very vivid color impressions, which seemed to be associated with sounds rather than forms. One of the interesting things he relates is that polysyllabic words, if quickly pronounced, give the color of the principal syllable accented; if slowly spoken, each syllable has its own color. The cases related by Francis Galton, in his well-known work, are sufficiently familiar and need only a passing allusion.

The case related by Rochas is that of a lawyer of 57, who, while he likes music, is not a musician. He is a traveller and an accomplished linguist, but has never heard of color audition as a phenomena, but always thought his experience an entirely normal one. He possesses this faculty to a remarkable degree and his impressions are entirely acoustic as to their origin and psychical. He does not exteriorize sensations, but sees, as he expresses it, the color in his brain. Of the vowels a is carmine; e, white; i, black; o, yellow; u, (oo) azure blue. The consonants are as a rule pale grey. Words take their color from the component letters. The various languages also have their color labels. Thus German, in which consonants predominate, is mouse colored; French, green merging into yellowish white; English is dark gray; Spanish has three colors, in the main, either yellow or carmine, but sometimes a dazzling metallic tint; Italian is yellow, carmine and black. It is with reference to musical sounds that this is a case of especial interest. The low notes are a very dark or deep rose tint; the more intense and higher pass from red to yellow, then blue, and finally black or a deep violet, precisely the order in the spectrum. The order is also according to the number of luminous light vibrations, thus the low notes of few vibrations produce red, while the highest, of many vibrations, cause the perception of violet. In singing the notes by names the vowels give the color. Low voices are of a deep carmine, while high sharp voices are a crude dark blue. Among musical instruments the sound of a bass drum is chocolate; of a trumpet, a brilliant yellow; hautboy, flute or piano, blue; violin and fife, deep violet or glossy black; guitar, gray.

Lauret acquaints us with the case of a man aged 50,

an intelligent scientist of normal vision, no musician, but likes music. His chromatic sensations are of acoustic origin and are always externalized. He fixes the colored image at a distance of one or two meters from his eyes. With him, as is usual in the case of letters, the dominating impression is produced by the vowels. Each image arouses not only a special color but a special form as well. Thus a is oval in form on a perpendicular axis of black; é and ê are squares of a dirty straw yellow color; i is a large point of silver white; o and ô are chestnut red on disks of madder red; u is a greenish blue. Diphthongs are colored like vowels: oë gives a circle, each half of a different color. Consonants have no influence upon the color of a vowel except in the case of m and n at the end of a word. Noises are also colored by this individual; thus the noise of the waves (French, vague) gives the color sensation of ou and i together. Deep voices are a dark chestnut if low and pass to light chestnut in louder tones. In singing the color impression produced by the baritone is chestnut passing into yellow; tenor is a deep yellow; contralto, light chestnut; mezzo-soprano, light yellow; soprano, passes into light and lighter yellow ending in a cream. Of the different instruments the clarionet produces in low notes a deep dirty yellow, while in the high notes it arouses a light yellow. The low tones of a flute are yellow, changing suddenly into light blue and then into white; hautboy, chrome yellow; the piano, if played moderately quick from low to high, gives first chestnut, then clear red, wavy yellow, blue, bluish white and white in turn; the violin gives garnet, orange, yellow and white in turn when passing from low to high notes, while in the same way the 'cello gives chestnut passing into carmine.

Prof. Steinbrügge of Giessen informs us of a case that was brought to his attention by an exile German Jesuit in Madeira. It was of a boy of sixteen, who had always lived in one of the country districts of the island, was strong and healthy, unimaginative, lazy and unmusical. His color impressions came spontaneously and suddenly; no time elapsed in which mere association could take place. It was rather a case of perceiving the objects, sounds and noises as actually colored. Fechner also collected a large number of cases, of which 347 were decidedly clear and authentic. Under these we find two of persons totally blind, who never noticed this peculiar faculty until after they became blind, when they possessed it to a remarkable degree. Also the case of a color blind person is cited, but he associated only the colors, known to him, while red was lacking. One lady, who always had color impressions in connection with numbers, proper names and days

of the week from her earliest childhood, wrote in connection with the answer to the list of questions sent out to such persons: "My mother associated other colors and was always very indignant when I maintained that the colors were other than those she gave."

The subject of whom Paul Raymond writes is a man of 30, strong, never been sick, but impressionable and has alternative moods of excitement and depression. From the earliest times he has had these color impressions called up chiefly with the vowels. No musical sounds give him perception of color. Each syllable takes its color from the vowels. The languages are also colored. Thus English is gray; Italian, yellow; German, black; Spanish, blue. But each word takes its color from its component vowels no matter in what language the words are found.

The case related by Grüber possesses nothing especially new. It is a case in which these pseudo sensations can be traced both to acoustic and optic origin. Figures also have colors with this person, and merely by the aid of combinations of colors alone is he able to carry on arithmetical processes.

Of the printed cases in English that which Prof. Holden records of his daughter Mildred is one of the most interesting and thoroughgoing. (Cf. Nature Vol. 44 p. 223σ.) By mere accident he learned of the color associations possessed by his daughter and became intensely interested. He made a record of the colors as associated with letters at six different periods, when she was 7, 8, 10½, 13, 14½ and 17½ years of age, respectively. The agreement of these different lists is most remarkable, showing very plainly that it is not a case of mere memory, but one of vivid and permanent associations. In numbers, 1 is black; 2, cream color; 3, light blue; 4, brown; 5, white; 6, crimson; 7, greenish; 8, white; 9, greenish; 10, brown; 11, black; 12, cream color; 13, blue; 14, brown, and so on. Thus 11 has the same color as 1; 12 as 2; 13 as 3; 14 as 4; 15 as 5, etc. In going over the table we find that G, P, T, Z, 7 and 9 are green; A, H, 5, 8, V and Friday are white; C, O, S, U and Saturday are yellow.

We will not give place here to the contribution of President Jordan, because it has been so recently published and is very familiar to all. In this same article are also detailed some of the interesting experiences of Prof. Spencer, of Moore's Hill College, Indiana. The writer is under great obligations to President Jordan for the hitherto unpublished account of the following three cases. The first two were furnished him by Mr. Launcelot and Mr. Harris, of Lexington, Va., while the latter was received from Miss Woodward, a student in Stanford University. They will be given with the exception of a

few minor changes in the correspondents' own words. The young lady in the first case is about 18 years of age.

My sister, who had been amused at my color associations, jokingly asked Miss Julia B—— the color of A. She replied, "Brown, of course," and was surprised to hear that it did not so appear to everyone. "How could anyone tell whether a name was pretty or not except by its color?" On further examination she gave without hesitation the colors of most of the letters, there being a few the color of which she found difficulty in expressing in words.

A=brown.	J=black.	S=lead-color.
B=gray.	K=green.	T=*pale* yellow.
C=yellow.	L=brown.	U=blue.
D=gray.	M=green.	V=gray.
E=yellow.	N=green.	W=brown.
F=black.	O=red.	X=lead-color.
G=light pearl.	P=gray.	Y=purple[1].
H=black.	Q=green.	Z="gray & brown mixed."
I=lead-color.	R=red.	

She also gave the colors of a number of names of persons, indicating whether they were pretty or otherwise.

About two weeks after this I questioned her myself and found the colors of the letters, which she gave instantly, to be, with one exception,[2] precisely the same as those on the foregoing page which my sister had taken down. I also asked the color of a number of names of persons and other words. I at once discovered that the word took the color or colors of the vowel or vowels which it contained.[3] The color in the accented syllable predominated, and when a syllable was but slightly pronounced, its color was ignored altogether. Thus Ashley is brown only.

The name or word is colored according to its printed or written form, not its sound. Thus *Lila* is yellow and lead-color, while the *ine* in *Josephine* is lead-color and yellow—the former color, however, predominating in each case. When I asked the color of *Goethe* she answered blue, spelling to herself *Gurty*, but on writing the name and showing it to her, she said it was *red* and *yellow*. Yet the sound evidently plays a part, as the following list shows:

Lancy,[4] bright brown.
Lila, bright yellow.
Lina, nearly black.
Myrtle, color of y, a beautiful name.
Larkin, "wine-color brown."

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[1]Color of crape-myrtle.
[2]Y given as black in my sister's list.
[3]Cf. Galton fig. 68, which I have since seen.
[4]Sounded as in Alice—seems inconsistent. The *n* seems to figure.

Lilly, lighter shade-lead-color.
ine in Josephine, Lilly.
Rosa, bright red.
Lula, deep blue.
Ulla, lighter blue.
Alice, dull brown (a hideous name).
Amy, less dark.
Lettie, light yellow.

There are some exceptions to her law for coloring words, the following being those I could discover :

Buck, red.	Will, red.	Mary, green.
*Wirt, wine-color.	Willie, white.	*Cassie, green.
Bird, red.	Ruth, lilac.	Lizzie, green.

All of these except those marked with an * are names, chiefly in her family, with which she was familiar before she knew her letters. The other two, however, she had not heard until recently. Moreover, a number of her family names are colored according to her general law.

It is with names of persons that she chiefly associates color, but she gives the color or colors of any word according to its vowels.

She has three colors for sounds—yellow, red and dark. A high note on the piano, a cricket's chirp are yellow; a low note, a man's deep voice, dark (precisely as with myself). She gives as red a cat's mew, a cock's crow, a man's whistling. A rattle is yellow.

She has no associations for tastes, smells or pains.

The second is the case of Miss Rosa B——. These two young ladies are cousins.

a=brown.	j=lead color.	s=yellow.
b=blue.	k=light brown.	t=yellow.
c=gray.	l=black.	u=blue.
d=lead color.	m=green.	v=gray.
e=yellow.	n=gray.	w=brown.
f=brown.	o=red.	x=black.
g=yellow.	p=yellow.	y=lead color.
h=green.	q=green.	z=gray.
i=lead color.	r=dark red.	

"U" when connected with "H" is always green; if not is blue. The color of a word is generally determined by the vowels it contains.

Names that she was familiar with before she learned her letters are exceptions to this rule.

The third case is in the form of a confession by Miss M——.

I think I must have been about four years old when I began to associate color with words, and it was evidently their sound and not their meaning that aroused the sensation.

Yet I do not remember that letters or numbers conveyed any such impression. Only new words such as I heard for the first time, gave me a distinct idea of color. Family names as well as given names nearly always had some color, and those which had none were soon forgotten by me. I can recall the name of some people in stories told me when I was a child and even now their color accompanies them. Again, I may think of a story and a person in it in whom I was as much or more interested than in those first mentioned, yet I can not think of the name. When the name is told me the color is lacking or indistinct. The same word or name always bore the same color, no matter where it was used.

Words which I had been accustomed to hearing daily had no color. At one time my brother, who could speak French, pronounced the word "encore," but without giving any meaning; it sounded yellow to me and I did not forget it.

I don't think that music was ever accompanied by color, at least not a melody, but I think that certain chords had color. I recall those words as having a distinct color, England, blue; Bertha, blue; Robert, brown; alligator, green; hyena, yellow; Alma, orange; Emma, blue; Charlie, red.

In the April number of the current volume of the *Revue Philosophique*, Beaunis and Binet publish results of their endeavor to measure the reaction time in two cases of color audition. The time indicated is that which elapses between the lettering of a certain letter and the touching of the electric key as soon as the associated color is announced. Even with their imperfect apparatus the time is found to be remarkably short. Thus in one case the average for a is .47″; e, .62″; i, .43″; o, .49″ and u, .56″, making an average of .51 sec. On the other hand the mean time for the recognition of the letter alone regardless of color was .45 sec. Thus the mean time occupied for the association of color was .06 sec. Phillippe made a series of experiments in which the mean reaction-time for figures alone was greater than for the associated color; for the person to name the figure it took 0.76 sec., to name the color 0.70 sec. The color was always seen before the meaning of the word was observed. In moments of fatigue the color impressions are more intense.

A considerable number of cases have been investigated by the writer, but only one is selected for a detailed statement on account of limited space. It is that of Miss S——, a young lady much above the average in intelligence and very accomplished. The æsthetic element is a prominent factor in her psychic life. She is a skillful musician, having taught in a conservatory of music for some years; very well informed as to literature and is herself a pleasing writer. With her

the color impressions are produced in three ways. First, and chiefly through the graphic forms; second, through auditory stimulation; and third, by means of ideation. It is a complete and fully rounded out case. *All* the letters are colored thus.

A=opaque white.
B=dark cactus green.
C=pale yellow.
D=tan color.
E=warm grey but pale.
F=very dark brown.
G=yellowish bright tan.
H=red, crimson.
I=black.
J=black sometimes shades into green.
K=red—very like H.
L=black.
M=blue.
N=gray.
O=black on white ground.
P=bright yellow.
Q=Naples yellow (buff).
R=dark green.
S=light green.
T=red; less intense than H or K.
U=gray.
V=pearl—slightly lavender.
W=black.
X=red; still less intense.
Y=yellow into green.
Z=brown sometimes shading into an iridescent purple.

The numerical digits are also colored. Thus 1 is black like i; 2 is opaque white, like a; 3 is bright green, slightly yellow; 4 is seal brown; 5, black; 6, grey; 7, yellow; 8, pink; 9, brown, lighter than 4. The colors of numbers are often and even generally more intense than those of letters.

In music written in different keys C. D. etc., the music has a general background of color which is the same as that of the index letter indicating the key. Thus music written in the key of D is tan color. All "sharp" keys are brighter and "flats" are less brilliant.

Words pronounced alike but spelled differently have *different* colors. Words generally take their tone of color from the *initial* letter. Thus with the same letters in different combinations we have different color-impressions, e. g., deer=tan; reed=greenish yellow. With this person we find there is an intimate relation between form and color. In grouping several letters of one color we find that H, K, T and X are red; B, R, S and 6 are green; C, G, P, Q, D, tan color; Y and 7 are yellow; Z, F, 4 and 9 are brown; N, U, V, A and E are gray. This fact we will refer to again in an attempt to explain these phenomena.

B.—Explanatory.

Believing that the scientific method is the only legitimate one, we have sought in the first place to present the facts as found before advancing any explanation. The disclosure of

facts is ever the best critique. First describe, then explain is certainly the best law for method of procedure. Before submitting our own view we desire to present a sort of résumé of the explanations already advanced.

Some have sought to find their explanation in the contiguous relations of sensorial centers on the cerebral cortex; in anastomosis between their different nerve fibres; in the reciprocal agitation, motion and disturbance of their individual cells. Others believe it to be due to reflex action and still others attribute it to a confusion of ideas.[1]

Carnaz held that it is a *visual* trouble, a hyperesthesia of the sense of color—pathological and due to some optical lesion. Prof. Wartmann and Dr. Marce also accepted this as an explanation.

Perroud (in 1863) was the first to say that it is not at all a pathological condition, not depending on material lesion, neither constituting an illusion or hallucination.

Chabalier, while he recognized the physiological explanation of the phenomena of pseudo chromesthesia, does not consider the phenomena as pathological, but allied to a light confusion of ideas and still more regarded it as a sort of psychic perversion, "an illusion belonging to that class of illusions compatible with reason."

H. Kaiser presents the idea that the subject himself puts the color into an intimate relation with the words in order to better engrave these words upon his memory. He says that it partakes then of the nature of association of ideas, going back to the earliest infancy and in virtue of which certain colors would be united to words voluntarily and then by long custom become spontaneous. Schenkel also offers the same as an explanation.

In 1875, Lussana wrote that the sensorial centers of sound and color in the human brain could be contiguous and thus influence each other in perceiving. Prof. Nuel has a kindred view. A year later, 1876, he writes that false secondary sensations might be due to a central nervous irradiation deterring the sensorial afferent currents. And still in the same line Pouchet and Taurrieux hold that it is due to an abnormal crossing of certain afferent or sensory fibres. But to this Pedrono objects that, according to the Young-Helmholtz theory, an immense number would have to be turned aside in order to do the great amount of work. This assumes the validity of the theory of Young-Helmholtz. Pedrono himself would rather admit the existence of sensorial cerebral centers located somewhere in the gray cortex and would explain on the basis of the contiguity of the chromatic and acoustic centers. This

explains only a portion of the facts—those of color *audition* alone.

Baratoux, in 1883, states that the chromatic center can be excited, not only by impressions from the retina but by deliverances through the other organs of sense. Is the stimulus always directly carried to the chromatic center or via the auditory fibres at times? He thinks these pseudo sensations of color due to anastomose of fibres, rudimentary in ordinary men, but in certain cases highly developed.

Prof. Steinbrügge maintains that this class of phenomena arises, in the earliest youth perhaps, from direct double perception. In later years the corresponding disposition of the brain which the double perception aroused has been impressed once upon the organic memory so that the associated color returns when the vowel or word is recalled. He says there are two possible explanations:—either the sensory stimulus of one sensory nerve passes to another sensory nerve in the course to the brain and thus reaches a cortical center other than that for which it was headed, or it spreads out beyond the limits of its own proper center, reaching a second center, by means of which a second perception is freed.

Rochas explains by assuming a sort of unconscious connection of cortical cells that have to do with hearing and vision. He adduces the case of Gautier, in which certain excitants, e. g., hasheesh, can establish such connections or relations which do not ordinarily exist. Feré believes colored vision to be entirely due to the particular tone of the nervous system, which is obtainable by different excitants and presentations. He rejects the theory of anastomosis between the two cortical centers and tangling of fibres. Professor Urbanschitsch considers these pseudo sensations as *reflex* sensorial phenomena.

Prof. Stevens' theory may be taken as a type of that large class who rest entirely upon "psychical association" as basis of explanation, when he says my own explanation of the matter is this: When we are learning to spell we associate certain colors with certain words and those words give us the idea of color. These words may be said to be chromopoetic and this property cannot be dissociated from them. For example, D is associated in my mind with dog, and when I think of a dog it never is a white dog, but always a black one; hence, D is black, I brings up ink and black ink; J, a jug of a brown color; V is a vulture, which I always think of as brown."

Thus there are a large number of investigators who claim that the physiological does not fill the bill, and that the only explanation is to apply the law of association of ideas and

they do it with a vengeance. They ask, why could these phenomena take place in the blind, in the darkness, when the eyes are open or shut, when the appearance of the colored image is equally sudden and spontaneous? But to our mind there are objections just as insuperable against the associational theory. While we cannot accept the purely physiological explanation, we can neither regard the theory of "psychic association" as satisfactory. The facts themselves present difficulties in which such a theory cannot over-ride or surmount. Before making a statement of these difficulties we wish to call attention to the general fact that as yet no settlement has been effected with reference to a theory of color. The Young-Helmholtz theory cannot account for some of the simplest facts, e. g., the phenomena of contrast. The evidence from histology is mainly against this theory. There is a tendency at present to accept a "four color" rather than a three color theory. Hering's hypothesis seems also to involve certain unwarrantable assumptions. His assumption of the "destruction" and "construction" of the visual substance is the most difficult article of his "color-creed" to believe. We adopt the conclusions of von Kries rather than those of Wundt, Hering or Helmholtz. After a careful and painstaking examination he finds it necessary to say that "the photochemical facts compel us to adopt a theory of component elements rather than one of changes qualitatively alike and arranged in a continuous series. Only by aid of assuming the varied combinations of such elements can we explain the phenomena of exhaustion, contrast, the mixing of colors from fundamental color tones and the phenomena of after images. The articulation and adjustment of these combinations we would assign to the central organs. We wish now to call attention to the following facts of pseudo chromesthesia.

In the majority of cases it seems to attach itself to a special condition of the nervous system, as well as to a well developed faculty of the imagination. Very rarely is there a defect of the eye or ear. It is remarkable that the younger Nussbaumer was equipped with a very keen ear, for he was able to distinguish eleven partial tones in a klang without using resonators, and without any practice. To be able to do this, of course, requires not only intact end organs of sense but also perfect condition of the central organs. Most of these phenomena, with notable exceptions, however, date from the early years of the subject. The larger number color sounds, especially speech. Some color *only* the graphic forms of letters. The secondary impression is so intimately bound with the perception that it is impossible to separate the two.

Then heredity plays an important part. It is *very infrequent* that a single member of a family alone experiences pseudo chromesthesic impressions. These impressions of colors become more intense, vivid, and striking when the person is fatigued. The intensity and clearness of these color sensations are different with the right and left eye of the subject. There seems to be a perfect agreement in the testimony to the effect that it is the form or sound and *not the meaning* of the word that calls up the color. To a certain extent these phenomena can be produced artificially by means of drugs, etc. The larger number of the subjects are women, who as a class can hardly be called *introspective* ; at least they are less so than men—but they are more observant. These secondary color impressions remain constant in their relation to the primary perception. That is, the same colors are always called up by the same excitant or stimulus. In experiments we have made upon such subjects with view to ascertain the reaction time, we find no measurable difference between the time it takes to announce the letter and to announce its color. Thus with the ordinary "Fallapparat" connected with a Hipp chronoscope, you expose the letter D. It takes no longer for the subject to say "tan-color" upon seeing this object than it does to say "D." There are *very few* exceptions to this rule. Indeed there are many cases in which the color is recognized before the letter or figure.

To us it seems plain that the theory of "psychic association" cannot account for all the above facts even if it may be satisfactory as an explanation in a few cases. We must, for sake of convenience, divide the phenomena into two classes. (1) Those that occur within the limits of the same sense, pseudo chromesthesia of optical origin, and (2), those phenomena which find expression in a different sense from the one primarily excited, for example—color audition. In no case would we feel warranted on the basis of our present knowledge in attributing these phenomena to the retinal elements—to the rods and cones—though there is a temptation to do so, especially in those cases where similar graphic forms produce similar color impressions. It may be that in some cases the phenomena could be rightly attributed to the crossing of certain afferent or sensory fibres. Pedrono objects to this by saying that it would take such a large number of such fibres. He seems to be ignorant of the fact that the number of optic fibres alone is between four hundred and thirty-eight thousand (Salzer) and one million (Krause). The optic nerve fibres lie in layers, but in the region of the *macula lutea*, they are finer and in fewer layers and anastomose freely. The rods and cones, it may be said in this

connection, are estimated to be seven times as numerous as the optic nerve fibres.

While it may be that a few of the cases can be best accounted for by applying the theory of psychic associations as in the case of number forms, we are inclined to attribute the majority of cases to the cerebral centers themselves and only on the basis of the facts involved. We might even say that the majority of those cases regarded as purely "psychic" can, in the last analysis, be traced to causes most intimately related to the cerebral centers themselves. For example, in the case instanced. Did Prof. S. come to regard D as black because he thought of a black dog, or did he think of a black dog because the letter D appeared black, and thus, as the initial letter, gave the color to the word? Why are there no cases of *dissociation* of these color impressions, as is often the case with purely psychical associations, such as some of the number forms? Paul Raymond instances certain clinical cases which go to show that there is a close relation or connection between the cerebral centers. It must also be remembered that, *anatomically*, almost every pathway is open to all the incoming impulses, and that the cerebral cortex is a physiological continuum made so by the so-called "fibers of association." The auditory and visual centers are the closest to each other of any of the cortical centers on the brain surface. As to whether there is any such thing as a "chromatic center" we know only the following: With reference to the perceptions of light, color and form, it occurs, of course, that the loss of light perceptions necessarily involves the other two—but either of these alone—form or color may be lost independently. Noyes cites a case from Graefe's Archiv of a person who could read words, but not count figures. The hemianopic loss of sight had respect only to perception of form, but not to perception of light. There was also a slightly reduced color sense in the remaining half of the fields. We know that the centers for light, form and color sense are all in the cortex of the occipital lobe. Wilbrand and Rheinhart place them one above the other; the light sense external, form sense intermediate, and color sense internal. Sequin, Verry and Nothnagel think them to be side by side. It can easily be seen that in any case the nervous afferent impulse could either by increased intensity, inhibition or irradiation call up a secondary impression. This relation is more frequent, when not within the limits of the same sense, between color and sound, because these centers are adjacent. On this so-called physiological basis we can explain those cases artificially produced as well as the intensifying of the color impressions

through fatigue. In other words, in such cases, the amount and nature of the blood supply has undergone a modification. After going carefully over the data furnished by several hundred cases of persons who possess the faculty of pseudo chromesthesia, we sum up our position as follows: Some few may depend somewhat upon the association of *ideas* dating from youth, developed in a manner conscious or unconscious, and thus ordinarily we may be said to arrive at the coloring of the days of the week, epochs of history, etc., similar to the phenomena of number forms. Even such are called "*automatic* associations." What is the real difference between perceptions through a sensation and one through an "*automatic* association?" In adult life we have no such thing as pure sensation. The content of our perceptions is supplied by the afferent impulse and the reproductive brain processes aroused. Every perception of a thing or quality is the sensation plus remembered sensations,—generally, organically or physiologically remembered—at least the process is a sub-conscious one. In the greater per cent. of cases the pseudo chromesthesic phenomena arise from some sort of cerebral work which is the outcome of the close relation of the cortical centers, which are connected by numerous associational fibers, notably the visual and auditory centers. Whether this is done by anastomosis of fibres or irradiation, or by direct stimulus of the fibres of associations, it is evident that in some cases at least it takes place within the centers themselves. It is a notable fact that the weaker the color impressions the more "psychic" and "ideal" it seems. There are still other cases which would have a certain analogy with optical illusions and still others that partake more of the nature of hallucinations.

The writer will continue his psychometric as well as other close tests and publish tables later.

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REPORT OF AN EXPERIMENTAL TEST OF MUSICAL EXPRESSIVENESS.

By Benjamin Ives Gilman.

(*Continued from last number.*)

The opinion which has been the starting point of this experiment is that music is a form of language, a vehicle by which thoughts and feelings may be transmitted from one mind to another. Musical compositions are said to be the texts which he who listens may read. In other words, a piece of music has, according to this view, a power to engender a more or less specific frame of mind and heart: this being its burden, message, import, or what it expresses.

In attempting to apply a test to the opinion thus formulated, our first business must be to decide upon what we shall take such phrases as these to mean; for they are far from constituting a definite doctrine. The general notion which lies at the basis of them is evidently that of the resemblance of the mental state of different hearers of a piece, or the same hearer on different occasions by more than the content of the auditory perceptions. Meaning by an individual instance of a certain music a certain occasion of the occurrence of a certain mass of auditory perception to a certain hearer, what we shall call an individual impression from a music will be the content of imagination and emotion entering into an individual instance of it. By a body of agreement or simply an agreement about a piece, we shall mean an element common to several individual impressions from it; and by a prevalent agreement, an element common to a majority of impressions. We shall here assume that any agreement which tends to be prevalent about a piece enters into its burden of expression. That is, whatever elements of feeling or fancy we have reason to think would, by taking more and more impressions, eventually prove to enter into a majority of impressions about it, will, taken together, constitute its probable burden of expression.

Our reason to think this of any element of spiritual content will emerge in a comparison of whatever impressions we can

obtain of the given piece. Left out of the comparison altogether, we may assume, by the terms of the definition of expression, will be, first: the impressions of inattentive hearers, for in these cases no auditory perceptions exist, in the sense of *grasped* content of sound ; and second, those cases in which the attention of the hearer is altogether absorbed by his auditory perceptions, for here no impression exists of which an account can be given. Certain impressions of attentive hearers having been obtained, the next step will be to ascertain by comparison what prevalent agreements exist between them. These hypothetical elements of the expression of the piece must then be tested by drawing new impressions into the comparison. Any hypothetical element which pre- vails in the new collection of instances, gains through

ERRATA IN ARTICLE ON MUSICAL EXPRESSIVENESS.

P. 44 l. 2, from top: for "apperception" read "appreciation."

P. 44 l. 5, from top: for "the auditory" read "this auditor's."

P. 47 l. 16, from bottom: for "of Beethoven Prelude" read "of the Beethoven Prelude."

P. 50 l. 15, from bottom: for "(of *E*)" read "(cf. *E*)."

P. 51 l. 25, from bottom: for "or morning" read "of morning."

P. 54 l. 9, from bottom: for "cloisterers" read "cloisters."

P. 56 l. 2, from top: for "B sharp" read "B natural."

P. 58 l. 1, from top: for "devoted" read "most devoted."

P. 59 l. 1, from top: for "d b" read "d flat;" for "b sharp" read "b natural;" and for "e sharp" read "e natural."

P. 63 l. 3, from top: for "acknowledged it in" read "acknowledged in it."

P. 65 l. 7, from top: for "cast out" read "has cast out."

P. 69 l. 22, from top: for "cause" read "a cause."

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it seems antecedently very probable would increase the ex-

REPORT OF AN EXPERIMENTAL TEST OF MUSICAL EXPRESSIVENESS.

By Benjamin Ives Gilman.

(Continued from last number.)

will emerge in a comparison of whatever impressions we can

obtain of the given piece. Left out of the comparison altogether, we may assume, by the terms of the definition of expression, will be, first: the impressions of inattentive hearers, for in these cases no auditory perceptions exist, in the sense of *grasped* content of sound ; and second, those cases in which the attention of the hearer is altogether absorbed by his auditory perceptions, for here no impression exists of which an account can be given. Certain impressions of attentive hearers having been obtained, the next step will be to ascertain by comparison what prevalent agreements exist between them. These hypothetical elements of the expression of the piece must then be tested by drawing new impressions into the comparison. Any hypothetical element which prevails also in the new collection of instances, gains through that confirmation the rank of a probable element of the expressiveness of the piece; any found among the minority only becomes through this fact a discredited hypothesis.

An agreement which is dependent only upon particular experiences of the auditors would certainly disappear in the long run, and may be thrown out at once. This would eliminate any agreement which could be ascribed to external associations with the music; such as those arising from its occurrence, or the occurrence of music like it, on special occasions, or at special places, or in connection with special things or events. Here belongs the requirement that the selection which is to be the subject of a test must not be *recognized*, else such associations (its name, the words that go with it, the dramatic situation for which it was written, etc., etc.) will give it an adventitious expressiveness. In this category are to be placed agreements caused by the communication of the spiritual state of one auditor to another. Any consensus caused by word or sign, or by the knowledge in any way received of the interpretation given by another, would be ephemeral, and is discredited in advance. Although the auditors may strive to disregard all such influences, it is a feat of introspective chemistry of which, doubtless, none are capable to dissociate their external accretions from the rest of the spiritual state. In searching for the expression of a piece, the aim should be to select the impressions of those auditors only to no group of whom such knowledge about and associations with a piece are common.

It may be that an inquiry like the present will bring to light certain conditions of mind, or certain types of mental structure, among instances of which the expressiveness of a music is more fully and distinctly felt than under other conditions and by other minds. One psychical element, which it seems antecedently very probable would increase the ex-

pressiveness of a given music to its possessor, is that of apperception, enjoyment of the auditory content presented. This may in part be an effect due to the attention, which, without interest in a music, would tend to desert the sound and reduce the amount of coincidence between the auditory impression and those of others toward that of any juxtaposed individuals. The impressions of those who do not enjoy a piece would, in so far as this is true, fall under the head above referred to, of inattentive states of mind, not to be called impressions. The effect may in part also be due to the fact that an enrichment of the mood of mind, a more prolific development of feeling and fancy takes place under the awakening and stimulating effect of auditory pleasure. If this is not to exhaust the matter, if apart from attention and excitement the enjoyment of a piece makes it more expressive to the listener, the fact is interesting and piques the curiosity. In regard to mental type. It is said, and doubtless with reason, that certain persons possess, and others lack, "a sense for the language of tone." A vague phrase like this being of no use whatever in any exacter inquiry, we are again obliged, in order to have something to test, to invent a definite meaning for it, as we did in the case of expressiveness itself. This will be the following: An auditor possesses a sense for the language of tone, or is specially sensitive to musical expressiveness when his impressions are a better indication of the probable expressiveness of a piece than are those of others. To express this more fully, the probable expressiveness of various pieces being determined by finding prevalent agreements about them, the examination of the degree of fullness and clearness with which a given burden is reflected in the impressions of a given listner, extended to various listeners and various expressive pieces, may reveal the fact that this degree tends to be markedly higher in the case of some than in the case of the rest; by which is meant that, comparing impressions from more and more pieces, the average of completeness and clearness with which their burdens appear in the impressions of certain listeners will eventually be found much higher than the same average in the case of the others. In default of any convincing evidence of the existence of such a class we can only make surmises as to the peculiarity of mental structure which may be its distinguishing mark. We shall, nevertheless, postulate some connection between this characteristic and powers of musical invention in so far as to assume that composers of rank possess it; that is, we shall assume that the type of mind which is capable of creating structures of tone which the world recognizes as beautiful, will also be specially sensitive to the expressiveness of such creations.

As to the general nature of our inquiry we conclude, then, that its aim should be to find prevalent agreements among impressions from music: these when verified becoming the probable expression or import of the piece in question. Only the impressions of auditors neither inattentive to nor absorbed in the music are to be relied upon in the test. The auditors are further to be independent listeners, who do not recognize the music nor together associate it in any way with particular experiences. We are prepared to restrict our attention to impressions showing an enjoyment of the music played, and to find indications in the replies that there may in different individuals exist a difference of capacity to detect the expressive burden of music.

These logical requirements of the present inquiry we have endeavored to meet first by making our experiment the test of certain hypothetical conclusions as to the expressiveness of certain fragments of music based on opinions already expressed in regard to them. In general this preliminary evidence is the interpretation of a single auditor and may be looked upon as his prevalent impression formed from repeated hearings of the piece in question. To some extent these conceptions are very likely the result of association, due to the auditor's knowledge about the various selections : to what extent their comparison with the replies will give an idea. If the evidence here presented be thought hardly more than suffices for the formation of hypotheses as to the burden of the selections in question, at least the first step has here been taken toward providing an inductive basis for an opinion upon the matter. In the second place the listeners have been chosen for the most part among those specially interested in music ; what the selections were has not been divulged to them, and they have been requested to signify the fact of any recognition of the pieces and to refrain from any communication with one another about them.

Independently of the general popular belief in the expressiveness of music, and in advance of a special investigation like our own, we have good reason to think that at least general forms of prevalent agreement tend to some extent to emerge among the impressions of different listeners to the same piece. The mechanism of the influence of tone in the psychical life is largely a mystery (e. g., its effects upon animals and nervously weak people), but there is one form of its working we *can* understand and from which a certain expressiveness seems very naturally to follow. A piece of music is a flow of sensation having certain characteristics which suggest other like things, and those would often in different minds be broadly similar. The illustration nearest

at hand is that of a decided double rhythm of accent marking equal lapses of time. This bears a general resemblance to the rhythm of ordinary human locomotion and its presence in a music, we may be sure, will make images akin to this present themselves very generally in the minds of listeners. So a marked triple rhythm will suggest ideas nearly or remotely like those of leaping and dancing. In the sphere of emotion there exists one specially marked instance of a prevalent effect. Whether the connection between the interval where ratio may be expressed by the fraction $\frac{6}{5}$, and the emotion of melancholy is to be explained as a recondite case of suggestion by similarity or in some other psychological way, or must be accounted for by nervous laws, is as yet undecided: but the connection itself is unquestionable. This interval is about $\frac{2}{5}$ of a tone less than another harmonious combination of pitch, viz., the interval $\frac{5}{4}$. Both are called Thirds (the name growing out of their span of notes in the diatonic scale), the interval $\frac{6}{5}$ being called the lesser and $\frac{5}{4}$ the greater Third. But so intimate is the connection between the lesser Third and feelings of sadness that the adjective "minor," by which the interval is known in English, has been taken up in our common speech as a synonym of "sorrowful." The phrase "a minor strain" often contains no reference at all to the interval of the lesser Third, meaning simply a strain of sadness, a strain engendering melancholy emotion. Some real expressiveness, therefore, we may assume that there undoubtedly is in musical forms. An inquiry in regard to the opinion which has been our starting point is accordingly to be viewed as an attempt to determine the limitations of a power of tone in the spiritual life of whose existence in some form we may feel assured in advance.

Let us now see how it fares with the opinions we have gathered about the different selections of music constituting our programme when confronted with the evidence furnished by our company of listeners. And first, it is to be remarked that as determined by its permanent record a piece of music may be said to be only a general scheme of auditory content of which the concrete embodiments given in different performances may differ very considerably. What we are here testing is the expressiveness which resides in these general schemes of sensation, determined by the printed record of music and not the expressiveness which compositions may have under the hands of special performers. The plan of a structure of tone left in Beethoven's Mss. of the Sonata Pastorale, may have been carried out in the performances from which Gurney received his pronounced impressions of passionate movement in a way differing in important charac-

teristics from that chosen in the performances which our listeners heard. Whatever negative results emerge from our experiment are therefore to be interpreted as indicating that it is not in the printed page that the supposed burden of the music resides—whether in a given rendition or not it would require further experiment to decide. It may be mentioned that the understanding with the performers was that the rendition should be to use the technical terms "objective" rather than "subjective"—that is, they should take no liberties with the text and should not be governed by any ideal or emotional mood unwarranted thereby. Further, the expressions of opinion on which the questions were based were purposely kept from their knowledge. A second point should here be noted. Only in the five pianoforte selections (I. III. IV. V. VIII.) can even the recorded scheme of tone, that is, the piece as written, be said to have been exactl reproduced at our concert. In the six vocal selections (II. VI. VII. IX. X. XI.) the voice was represented by the violin and the accompaniment, if at all by the piano. In these cases the test is then of a somewhat different texture of tone from that of which the selections as recorded consist, and on which the preliminary evidence was presumably founded. The instances in which a more ambitious representation was attempted proved comparatively barren of result and are not included in our evidence. Let us now proceed to an analytical comparison of the notes on each piece judged, both among themselves and with the utterance on which the question was founded.

I.

My conception of the burden of Beethoven Prelude may be analyzed as follows: (a) amid deep gloom; (b) intense labor; (c) repeatedly; (d) directed; (e) toward a single achievement; (f) without progress.

(a) No one except, perhaps, *I*, agrees with me in finding the piece *deeply gloomy*.

According to nearly half the listeners it is more or less tinged with depressed emotion, viz., M (melancholy); *E* (disheartening); B, D, J, K (sad); E, *G*, *J* (unrestful); P (yearning); *H* (regretful); A (weird). It is apparently neutral to H, L, *F* and *K*, and either serene or buoyant to I, F, N, O, P, *A*, *C*, *D* and *L*.

(b) No one states as unequivocally as I the *intensity* of the energy involved, although the notes of E, L, M, *A*, *E*, *G* and *I* may be conceived to imply it.

It is especially noted as *mild* energy by A, H and O.

To F, D, K, *H* and *L* the images suggested can hardly be said to have involved activity at all.

(c) All of the twenty listeners to whom the piece unequivocally signified activity, regarded this as *repeated* (I take it waves were present to *A*'s mind).

This idea of the aggregation of like elements is lacking in the impressions of the other five, F, H, *H*, *L* and K, the latter especially remarking that the structure of the piece is organic and not that of accretion.

(d) These twenty listeners gave eight judgments affirming the activity to be *purposive*: so E, *J* (argument); H (parting branches); O (fashioning); L (conquest of an opposition); M (struggles of life); *D* (successful energy); *E* (flight against wind).

In fourteen it is without purpose: so E, P (dance); A (tree-tops); B and I (chimes); P, *C* (brook); J, N, *A*, *F*, *G*, *I*, *K* (waves).

(e) Of the eight who find the suggested activities purposive, but two agree with me in thinking them attempts at *a single achievement*: so L (conquest of an opposition); and *E* (flight against wind).

They are two antagonistic efforts to E and *J* (argument); successive steps in a labor to O (fashioning); and *D* (successful energy); successive achievements to H (parting branches); successive struggles to M (career).

(f) The twenty listeners to whom the piece expresses activity give fifteen judgments, agreeing with me that the activity is *without progress*; or at least arrives nowhere: so A (swaying); B, I (chimes); E (argument indecisive and dropped); H (advance, but no arrival); J, N, *F*, *G*, *I*, *K* (waves); E, P (dance); *E* (flight against wind); P (brook).

They give nine judgments affirming progress: so E (torrent); L (opposition conquered); O (fashioning); *A* (haven); *C* (brook); *D* (successful energy); M, *G*, *J* (peace).

Besides the agreements thus far mentioned, there is in the notes another which deserves notice.

To several listeners the piece brings the suggestion of a church and its chimes or organ: so B, *I* (church and funeral); D (organ); I, P (opening of service); *L* (wedding; organ). This is doubtless due to the polyphonic character of this prelude (which two listeners thought was by Bach), organ compositions having commonly such structure. These ideas are accordingly special *associations*, and are to be thrown out in a quest after the real intrinsic import of the piece.

The various elements of my notion about this music have stood the test of comparison with those of others in the following way:

(a) Deep gloom. The opinions are about equally divided as to whether the piece is bright or dark in emotional tone. The indication accordingly is that, although in a minor key, it has no pronounced emotional tone. This is noticeable.

(b) Intense energy. The fact that some notes can be interpreted as suggestions of strong activity, while to others it is expressly stated as mild, and the rest seem to regard it as moderate, is an indication that the piece does not, in reality, express any particular degree of energy.

(c) Repetition. A large majority recognize this element.

In regard to (d) purpose or no purpose, (f) progress or no progress, it can be made out that the negative alternative prevails in both cases. This indicates that the element of purpose [and hence (e) determination of achievement] probably is not, and that the element of absence of any progress probably is, a part of the expressiveness of the piece.

In short, the indication of the answers is that this Prelude expresses simply *recurrent activity without progress*, whether grave or gay, intense or mild, purposive or purposeless — all being indeterminate. The determinations of those points in the above formula as well as in the judgments obtained are, it is indicated, mythical creations, which in my mind have grown up around the piece in the course of long acquaintance with it, and which in the listeners' minds have been evoked by the first impact of the music.

But if recurrent activity without progress is all the expression, import, spiritual content of the piece, it is a question whether it should be said to have any expressiveness at all, for it may be claimed that this much is *in* the music itself. It *is* recurrently active, and at least in the fifth bar from the end (beyond which its figure may be said simply to die away) comes round again to exactly the texture of tone that constitutes its opening bar.

We may, indeed, compare our result and the comparative lack of imaginative content in the main impression received by K (idea of organism), with the remark of C, that the make of the music absorbed his attention, and surmise that polyphonic music may, perhaps, in reality be less expressive, more musically absorbing, than melody or harmony.

II.

Gurney's characterization of the melody from La Favorita involves the two elements of weakness and absence of rigidity (flaccidity). Any definite suggestion of weakness is hardly found in more than seven replies: so K (feeble nature); L (dependence); N, *G* (faint hearted love); O (discourage-

ment); P (gentle sadness); *F* (hesitation). The element of flaccidity may be thought, perhaps, to appear in eight cases: so E, P, G (plaintiveness); H? (melancholy); I (relaxed body); L (dependence); O (discouragement); *K* (sad meditation). The result may be said then to be the negation of Gurney's interpretation in both points.

In so far as a setting by Donizetti is not to be regarded as a somewhat hap-hazard selection from his note-book, we can take the operatic situation for which he wrote as an indication of his own conception of his work. The words of "O, mio Fernando" are an expression of passionate regret, a resolve of sacrifice, and a cry for death. The situation is La Favorita's choice to give up the man she adores and who adores her, rather than deceive him by hiding the dishonor of her past life. Here regret, complaint and resolution take the place of flaccid feebleness; but these characters fare hardly better among our listeners. For regret and complaint may be cited A (liebesweh); B, J (despair); E, G, P (plaintiveness); F, I, *D* (regret); H? (melancholy).

A hypothetical burden for the melody, which a general survey of the replies suggests, might be formulated as *yearning*, or want of attainment. For this interpretation may be cited A, B, C, E, F, I, K, L, N, O, P, *A*, *D*, *E*, *F*, *G*, *J*, *L*, eighteen voices. But as in the last selection, it is a question whether this can be considered as *expressed by*, or whether it is not rather *in* the music. I mean by this, as before, that taking the content of pure *sound perception* which makes up the melody, leaving out any emotion, and leaving out any fancy which is not an image of a *sound*, we find therein an end set and unattained. This end is the *tonic-note* of the melody, which we may conceive as floating in the fancy of its hearers as a wished-for sound. The song begins away from it (on the note called the mediant), wanders about in the scale (of *E*) without letting us rest in this desideratum of the ear, and ends (in the fragment played) on the note above the tonic, leaving our sound-wish unsatisfied. The general scheme of the experience we call yearning is thus given purely in the sounds of the song, and something more must be claimed (e. g., an emotion which goes with an end unattained) in order to give the melody any expressiveness at all.

The phrase in which Gurney characterizes the melody from Der Freischütz contains beside the element of beauty (fair) three others—serenity, permanence and strength. We can find an atmosphere of serenity in, at most, six answers: so C (satisfaction, assurance, relief); H (serious joy); O? (confidence); *G*? (cheerfulness); *J* (attainment); *K* (serene joy). Permanence appears, perhaps, in E (joyful looking to

the future); L (hope); *K* (serene joy); and in the second poem cited by N:

> "But years must teem with change untried,"
> * * * * *
>
> "No fear! or if a fear be born
> This minute, it dies out in scorn."

Neither of these characteristics is therefore to be admitted. Some suggestion of strength may be found in sixteen answers, viz.: A? B? (triumph); H (sunlight); I (up and doing); K (strong nature); L, P (vigorous); M (sparkling breeze); C, N, O, *L* (confidence); *C* (impetuosity); *F* (resolution); *H* (energy aroused); *I*? (lively). We may, perhaps, count this latter consensus as a corroboration of Gurney, and assume that the melody probably does express vigor.

Calling in now Weber as a witness, on whose high standard of fitness between poetry and music we may safely rely, this element of vigor is again indicated. At the opening of the air (which alone was played), Max sings—

> "With a light heart I roamed through wood and field;
> All I caught sight of fell before my trusty rifle."

Here we have the suggestion of vigor, high spirits and independence; or morning hours too, and the open air.

Sixteen of our replies recognize an element akin to lightheartedness in the melody: so A (light-hearted); D (gaiety); E, F, G, H, K, *L* (joy); I, N, P (buoyancy); O (gay, nonchalant purpose); *A* (allegro); *E* (shallow buoyancy); *G* (cheerful); *I* (lively). Even suggestions of independence (cf. I, L, M) and daylight and the open air (cf. H, M, *E*, *H*) are not wanting. We may fairly add the element of buoyancy to that of strength, and as the result of our test claim *light-hearted vigor* as the probable burden of the melody.

III.

The opening of the Sonata Pastorale conveyed to Gurney the idea of *resistless movement*, all the more resistless after the interim of runs. Out of twenty-four listeners who gained some definite suggestion from the selection fourteen connected it with a movement or progress of some sort: so B (half-sung soliloquy); D (orchestral climax); E (a coming good); F (advent of spring); H (mild progress); I (railway train); N (flight into the blue); P (abandon); *A* (learning to walk); *D* (consent); *E* (advance toward a decision); *G* (dance); *H* (an uplifting); *K* (a rocking boat). The effect of power on the other hand is felt by not more than eight: we may perhaps discern it in C, J (invigoration); E (stress of impatience); I (railway); N (religious context); *D* (consent of many); *H* (divine uplifting); *E* (conflict).

According to our listeners, then, the impression of resistlessness is not in the music as written. We can regard it perhaps as a special product of the piece in Gurney's mind. Indeed while feeling this impression unequivocally, Gurney distinctly recognizes (p. 172) the possibility that others may not. Or again, this may be a case where an impression is the growth of a particular habitude of the rendition of the music. There are traces in our replies of the conception of a surrender to some power: so I (to rhythm of rails); *K* (to the waves); N (abandonment of finitude); P (festive abandon); B ? (serene confidence); *D* (surrender of many wills); *H* (sustaining power of faith). It is not impossible that were this conception in the mind of a performer, such impressions might become more frequent and be found to develop into the sense of compulsion of passionate movement that Gurney felt.

The number of persons who found a certain triviality in the music is worthy of note. Thus B (careless soliloquy); D (ephemeral feeling); E, K (pearl-color, powder, carved walking sticks); G (opera air); I (song chorus); L (trivialness); M (frivolous); N (jolly flight); P (gay responses); *A* (infancy); *E* (inconsequence); *G* (fête).

Perhaps after all *a happy surrender* is the most promising hypothetical burden to be derived from our replies: for surrender becomes triviality when the compelling power is slight and when it has grown great is the sign of its resistlessness.

IV.

Of the Chopin Ballade, Rubinstein writes: "Is it possible that the performer should not feel the necessity of representing to his hearers; a wild flower caught in a gust of wind, etc., etc.," intimating that this impression is to be given in the rendition. It has nowhere completely emerged in our replies, as a comparison will show. The conception may be formulated thus: A character of innocence and gentleness attracts the admiration of a bold wanderer whose, persuasion met by reluctance becomes violence that conquers by destroying. The story then opposes weakness and simplicity and fascination to strength and experience and desire; tells first of gentle means opposed inertly, then of harsh means opposed feebly; then of a victory that is death to the vanquished. The dramatis personæ are two, defenseless charm and imperious longing; the action is the attack and ruin of the former by the latter.

The closest approximation to this story among our replies is that of *E*; but here the actors are the worse and better elements of one nature. As in Rubinstein's interpretation

there are here two stages of the conflict, one in which the purer and weaker element still maintains itself, and a second and sterner struggle in which its downfall is complete. The last few bars where, perhaps, in Rubinstein's fancy, the fallen flower exhales its life, *E* has beautifully interpreted to mean the survival in outward conduct of the blamelessness the heart has lost. Next nearest perhaps is the impression of *K*, in which while all the movement in Rubinstein's impression has faded out, there remains his conception of two personalities, one who will not be gainsaid, however the other may expostulate and entreat. Two personalities, a stronger and a weaker, appear in D's suggestion of George Sand and Chopin, but certainly no masterful beating down by one of the opposition of the other, no mortal victory. The introduction depicts to *C* a personality at peace, yet it is not the calm of untroubled innocence, but that of resolute renunciation ; and it is this element which is the stronger in the conflict that follows, and upon which desire and entreaty exhaust themselves in vain. The framework of B's story (murder) is curiously like that of *C*'s, although the background of mood in the two is so diverse. The presto con fuoco represents to both a superior power opposing itself to wild entreaty, and in both it is the victorious element (and not the vanquished as with Rubinstein) whose controlled strength is suggested in the prelude ; only in *C*'s story it is the weaker and in B's the stronger element that is the aggressor. Further B brings in a third element, the march of surrounding event, with which we may compare the solemn tendency caught sight of by O. On the other hand, in N's story of the lover bereft as in Rubinstein's of the flower, the element portrayed in the quiet introduction is that which succumbs later to a resistless power. Yet it falls, not in a gradual contest with a personal force as Rubinstein has it, but to the instantaneous blow of some fate ; (in the poem, death) and the struggle is an effort to *win back* what was lost. One of the pictures (a battlefield) suggested to K is very similar to this : for here, too, the quiet introduction depicts peace (to be sure, sorrowful or monotonous), and the tumult is its destruction in a sudden calamity, and possibly to efforts (search) after its restoration. Happiness wrecked by a sudden calamity appears again in Browning's poem "In a Gondola" suggested to *I*. With these the other story suggested to K, that of Dido and Æneas, has in common a shattered paradise of love, and (excepting *I*) unavailing efforts to regain it ; here again it is the beloved who is bereft of her lover, yet the calamity is not external, but his own act. The trait of a struggle to regain something lost (N, K) reappears in O's

impression, in which while the calm introduction portrays maidenly innocence and peace as it does to Rubinstein, the tumult is an effort to *rescue* the heroine after some external calamity and not the progress of her downfall. Love is the burden of the introduction to A, as to N, K and *I*, but the lovers are together in disaster and together lost; together in disaster, too, are L's friends, but it is inward dissension, not outward misfortune. The scene of Ophelia's burial and Hamlet's struggle with Laertes came to *D*'s mind: not her tragedy that had then been played through, though this latter is much nearer Rubinstein's conception.

In several of the impressions only broad outlines of a drama of tumult following peace presented themselves: so *F*, D (passionate interlude in a quiet life); F, *J* (peace; conflict; rest); G, P, *I* (storm); I (a child's future peril dreamed by the mother); J (peace; catastrophe; ruin); K (sad monotony—disaster upon disaster; ruin); M (cloistered peace engulfed in disorder and surviving only in memory); *B* (struggle; success).

To some listeners only disconnected scenes were suggested: to C (devotional; storm; gaiety; stormy; gentle again); *A*, *G* (gaiety or peace; apprehension; grave joy; stormy passion; memory of gaiety, or peace again; P, *H* (cloistered peace and wordly tumult); *L* (storm and music).

Finally, in two stories cast in the same general mould, E and H give a faithful record of the surrender of the sense of musical charm to the sense of the ridiculous. The resistance of E to demoralizing laughter is no less edifying than the frankness with which he frees his mind about the selection. His hero's malformation and the dismemberment of H's heroines; fruit of the monotonous introductory rhythm and the riotous discord later—show clearly wherein the Ballade makes exactions upon the listener.

Summing up we find that the quiet introductory theme, identified by Rubinstein with a personal type of weakness, inexperience and charm, is in sixteen impressions interpreted as depicting a *condition of quiet*; so F, J, *J* (peace); K (sad monotony); P (a soul at peace); N, A, L, *I* (happy love); M, P, *H* (cloisterers); C, *G*, *L* (devotional); B (stealthy movement); and in but nine suggestive of character: so D (Chopin); *F* (youth); *D*, O (young girl); E (rich-minded sobriety); H (doll); I (infancy); *C*, *E* (sensitiveness and force).

While the presto con fuoco is generally recognized as a scene of storm or conflict, by eleven listeners the elements at war are not definitely distinguished: so C, G, *L*, *A* (storm); M, *G*, *J* (war); F, P, J (conflict); *F* (excitement). Of the

remaining seventeen listeners fifteen (all but L and *D*) recognize with Rubinstein a stronger and a weaker antagonist: so A (perils; lovers); B, *I* (murderer; victim); D (George Sand; Chopin); E (rapids; swimmer); H (boys; dolls); I (vicissitudes; individual life); K (Æneas; Dido; or a soldier's fate; his beloved); N (fate; a lover); O (misfortune; a victim); *B* (purpose; disappointments); *C* (resolution; entreaty); *E* (inclination; duty); *H* (the strong; the weak); *K* (desire; entreaty).

The stronger is a person as in Rubinstein's story in nine cases: so B, D, H, K, *B*, *C*, *H*, *I*, *K*; but his identification of the weaker with a character presented in the prelude occurs but three times: so D (Chopin); E (swimmer); I (infancy); identification with an element or elements presented in the prelude occurring five times: so *E*, A, H, K, N.

A definite ending to the struggle appears in twelve impressions: this is destruction in some form in seven: so A (wreck); B, J, *I* (violent death); K (despair); *E* (fall); O (sacrifice); a failure of destructive efforts in one *C*; a failure of constructive efforts in two N and K; and their success in two D (old life enriched) *B* (success).

The closing bars of the piece are interpreted in thirteen impressions as a return of some semblance only of what was pictured in the introduction: so A, *I* (subsiding sea); B (remorse); D (old life enriched); H (sole survivor); I (good-night); J (hush); M, N, O, *A* (happiness remembered); *D* (thought of the dead); *E* (outward purity).

It is evident that there can be found in these answers no prevalent agreement on any but a very meagre story. More can hardly be claimed as the conception of a majority than that of peace followed by a struggle of a stronger with a weaker element, which is renewed after an interval. All the rest of Rubinstein's interpretation the test has failed to confirm, thus corroborating the negative side of his opinion, viz., that this story is not contained in the music as written. Our replies possibly add as a hypothetical outcome of the drama the return of at most a semblance of a former peace.

But for the third time we meet the question whether, if this be all its burden, the music can be said to be expressive at all; for as much of a drama as this seems to be contained in the flow of sound itself that constitutes the piece; the formula simply enumerates certain characters taken on successively by the listener's auditory perceptions. The beginning of the piece *is* quietly monotonous as a sequence of sound. Later, there *are* in the auditory perceptions two elements, a stronger and a weaker, in conflict; the latter being the *image* of the diatonic scale in the mind of the hearer, which opposes the

introduction of notes foreign to it; the former being *sensations* of such notes (e. g., B sharp, the piece being written in the key of F) which enter nevertheless, and powerfully; sensation being stronger than fancy, we have an antagonist whch conquers, and an antagonist which yields. Finally the sound of the closing bars *is* a semblance, and only a semblance, of the quiet flow of tone constituting the introduction. According to this the present test not only negatives Rubinstein's dramatic image, but offers no suggestion of another. The piece may be imaginatively expressive and what is thus expressed may be the story of the flower, but whatever it be our replies have failed to indicate it. We must have recourse again to emotional elements, forming a prevalent progression, of underlying shades of feeling in the various images. The formula might be: peace (several find it clouded in some way); fear. To several the close is regretful.

The two characteristics of suggestiveness and expressiveness in music are to be differentiated. A music is suggestive when the tones come embedded in a rich mood of fancy and emotion; it is expressive only when we find these moods alike in comparing one with another. Hence, it may be said that music is never so suggestive as it might be were it not so expressive. A lack of expressiveness may spring from two causes; a piece may be too little or it may be too diversely suggestive. The negative results of inquiry IV. are certainly due to no want of response to the Ballade in the hearts of this company of hearers.

V.

The Andante of Beethoven's opus, 109, depicts a mood that brought to the point one hardly cares fully to express even in the interest of truth. One wonders whether there is another art whose product could do quite the same. The replies unquestionably confirm my impression that the fragment has religious significance. There is more unanimity here than about any of the other selections. In describing the atmosphere of the music, words of religious import are used in nearly half the cases: so H, I, J, L, M, O, *A*, *C*, *G*, *I* (religious); F (prayer); *H* (aspiration); K (worship). More or less closely allied impressions are G (a placid mood in the presence of the sublime); C (resignation); D (peaceful sadness); E (grave, not regretful); P (tender seriousness); *B* (seriousness of life); *E* (comfort in sorrow). More divergent are N? (calm dignity); *D* (placid retrospect); *F* (retrospect); *K* (quiet happiness). Three listeners detect a certain unrestfulness in the music: so B (doubt); L (unrest); *L* (restlessness).

One listener (M) remarks on the likeness of the piece to German church music. Admitting this to be the source of its religious impressiveness, the agreement upon this would then be of associative origin born of the familiarity of the audience generally with this form of ecclesiastical composition, and not a real message carried beyond the limits of this acquaintance. It would be a difficult enough matter to find listeners who should not have such associations and who should at the same time be capable witnesses: but had this been the source of the unanimity in the present replies, one would have expected to find in them more ecclesiastical or ceremonial references than the three we have: viz., besides M, K (Strasburg Cathedral); N (ceremony).

VI.

The comic element which Engel finds in Barberina's aria in the Nozze di Figaro can be detected in the impressions of at most five out of twenty-two of our listeners who report on this selection: so C (sad humor); E ? (uncouthness); N ? (no high tragedy); *H* (teasing); *L* ? (doll). This result (looking at numbers alone) negatives the idea that the trait in question appears in the music; and indicates as its origin the dramatic and poetic setting of the air, whose effect in the mind of one familiar with the opera is no longer to be surely distinguished from that of the texture of tone. But our conclusions must now, I think, be more than ever tentative, for the fatigue of the audience has by this time become a factor to be reckoned with. Further, we find that in one of the replies possibly to be adduced as evidence for an element of humor in the piece, the dramatic situation for which Mozart composed it is recorded almost as it might have been had the listener had the scene before him as he wrote. Barberina has lost the pin that the count gave her to carry to Susanna, and sings: "I have lost it; poor me; who knows where it may be? I can't find it; and my cousin; and the count, what will he say?" The impression he received from the music of this complaint N describes thus: "It is an impression of some one seeking in hope and sadness mingled for some lost thing; whether a lost child or a lost latch-key, I can't say." But it is evidently the latch-key, for there is "no high tragedy" in the music, and even so he has himself searched his pockets "for the last nickel and found it not." To N there was, in the music, not only a suggestion of search unsuccessful but of search with a certain element of triviality about it. But for the element of dismay in the words, one could hardly go further unless to designate the object as a pin, the loser as a serving maid, etc., etc., which details the

devoted believer in musical expressiveness could hardly demand.

This coincidence, which was due to no conscious recollection of the opera (heard, if ever, a dozen years ago, in Germany) on N's part, may be explained, if we please, as a feat of memory too recondite to give any recognizable account of itself. Or again, the particular nearness of the approach to Mozart's idea may be what we call a chance. The remaining alternative is to consider it a real striking of hands on the Elysian fields between Contemplation and Creation. There is a type of mind, according to this, Mozart and N being instances, to which music may have an expressive burden, as complicated, as the notion of an unsuccessful search after an unimportant thing. The premises by whose aid we draw this inference from the fact of this coincidence (we have had complex coincidences among our replies before) are two: the assumption (already made in Weber's case) that the aria of Barberina sprang into being out of a mood in Mozart's soul of vivid realization of the scene to be given a musical accompaniment; and the assumption that the mood of composition whose precipitate is a given music, is a better indication than that of any mere auditor of the retinue of spiritual elements with which the music will be apt to be accompanied. On these assumptions N is indicated as possibly one of the natures specially sensitive to musical expressiveness, of whose existence we have been prepared to find evidence.

But let us inquire what prevalent agreements exist among the other replies, and what, if anything, we can more plausibly add to them, by the aid of the principle just stated, as a part of the hypothetical expressiveness of Barberina's aria.

We shall, I think, in this way, reach an indicated burden which may be analyzed into the four elements of weakness, simplicity, desire and pain (meaning by pain simply disagreeableness, not physical pain only, nor only the intensely disagreeable). As to the origin of these various elements, the following hypotheses may be made: The weakness and simplicity are, perhaps, given in the want of intensity of the strains, their small range of movement in pitch and their small duration in time (iteration). The desire is given, perhaps, in part by the fact that the melody keeps away in great measure from that desideratum of the ear, its tonic note, beginning and ending on the dominant; and in part, perhaps, by some resemblance in its flow to the intonations of the voice in uttering a wish. The germ of the pain is already given in the desire, but there is unquestionably a likeness between the semi-tone changes which are prominent

in the melody (c″, d″ b; b′ sharp, c″; e″ sharp, f″) and the continuous change of pitch through a small compass characteristic of the gentle expression of pain by the voice (moaning); further, the fact that the music is the minor mode is another source of pathos.

The wind about a house to which B likened the music changes its pitch continuously through a small interval, like a cry or moan of pain. B used the former word; the latter is chosen by P, in whose impression a human suggestion enters. In J's picture of the expression of a diminutive anguish to the vocal utterance of pain, add themselves the ideas of weakness, simplicity and indefinite desire (crying child). Here *L* takes up the suggestion and constructs on it a little nursery story of a child's plea with her doll for affection. In the impressions of F and I, which also contain the idea of entreaty, this suggestion of simplicity and want of development falls out and the main image is one of desire and pathos (pleading for forgiveness). But developed into rusticity and uncouthness it, in its turn, is the principal part of the image of E, where the idea of entreaty is only to be inferred from an unwillingness and final consent that was heard in the music. A picture not unlike is that of *H*, where a certain simplicity, with a dash of irritation in it, appears in the notion of teasing, entreaties becoming simply questions. Questioning is the sole content of H's impression and *D*'s, and in the form of uncertainty enters into those of *C* and *G*. The latter, further, expressly mention *search*, the only one of the listeners, beside N, to do so. Pathos and weakness, with or without desire, enter into the impressions of A, M, *B* and *J*. In those of K and E the simple monotony of the music, with its touch of mild sadness, have become a resignation to the humdrum; *A* makes it even a sunny resignation, C a humorous sadness. In the reply of N finally the elements of simplicity and mildness manifest themselves through his tendency to suspect the woe, is a light affair, those of desire and pain in the search with grief.

Taking all the twenty-two replies together we find a marked prevalent agreement on the elements of pain and desire; fifteen or sixteen voices for each. Further, introducing the evidence of the dramatic situation, that is, taking Mozart as a hearer, whose opinion counts for more than that of any other, weakness and simplicity, since they form a part of Barberina's character and are each recognized by a sizable minority of our audience, become further hypothetical elements of the burden of the piece. Whether we should admit the element of search (active desire for something lost) supported by the agreement of the dramatic situation with but two of our

replies, is a question to be best settled by a new test of the melody with other listeners. But there remains a presumption that N belongs to a type of mind specially sensitive to musical expressiveness, for besides this doubtful element, his reply contains all those we have found reason to conclude, enter into the burden of the selection. This is true of but two others, those of J and *L*. The fact finally that one of these (*L*) may possibly, like N, be conceived to have felt, in the melody, something of the playful atmosphere under whose influence Mozart undoubtedly wrote, leaves us in doubt as to whether Engel may not be right, after all, in claiming that Barberina's song has comic traits. This is another point that only further experiment could settle.

Of these four elements the pain is certainly emotional; there is an emotional element likewise in the spiritual state we call desire. These two the music must be admitted to have as true burdens; they are not contained in the auditory perceptions. But the non-emotional side of desire appears to be a character of the music itself; and the melody certainly *is* a mild and simple structure of tone, although it may *express* these traits too.

VII.

The opinion of Gurney quoted above about the fragment from Händel is to the effect that an absolute dejection is already expressed in its first five bars, this character being mainly the contribution of the phrases we have called *b*, *c*, and *e*, *f*. These carry a suggestion of human movements of drooping and sinking, which is emphasized by the pause between them, as if one resisted momentarily only to give way more completely. This impression of pathos is recognizably deepened at the note g flat in the sixth bar.

Assuming that the melancholy character of this fragment would be generally felt, the question remains whether Gurney was right in regarding the descending thirds and the g flat as the main factors in this result, and right in surmising that the thirds act through suggestions of human movement. This is no longer the inquiry simply into the spiritual state accompanying music, on which we have hitherto been engaged. What we want to know now is, with what elements of a certain complex of tone the impression of sadness it makes (if it make one) is mainly connected, and what is the link between them. Not only susceptibility to impressions from music is here demanded and powers of introspection and expression capable of catching sight of and hitting them off in words, but powers of analysis able to trace an element of spiritual state to its source, in an element of auditory per-

ception, and of noting what it is in one that brings up the other.

In spite of the difficulty of this task, something of a consensus emerges from the replies and one that favors Gurney's analysis. To fourteen out of twenty-two listeners the phrase we have called *h* and which contains the g flat remarked upon by him, is one of the parts of the air most expressive of melancholy; and seven of these (viz.: O, E, I, C, L, *H*, *J*) find therein the culmination of this effect, three (A, *B*, *F*) finding it in *h* and *i* together. According to C the extreme dejection begins upon the g flat, and I calls it the decisive note of the piece; with the other flatted notes (c and d) its part in the impression of sadness is also remarked upon by K and M; the flatted c being specially singled out by *J*. About the rest of the fragment there is not so much agreement, but what there is points out i and the two passages of descending thirds as main agencies in the effect of melancholy; nearly half the listeners selecting these phrases. A few chose the others (*a*, *d*, *g*) and some regard the impression as a resultant of the whole texture.

That the burden of the piece might be thought either sadness or dejection was suggested in the question, and the opinions of the audience seem to be nearly equally divided as to whether or no it expresses hopelessness as well as melancholy. Beside the word dejection, hopelessness is used (L, *E*), discouragement also *D*, and abandonment (*H*). On the other hand, sadness alone appears to have been recognized by D, E? J? G, M, *A*, *B*, *F*, *K*, and a positive opinion against hopelessness is expressed by A, B, N and O. Nothing more than that the melody is deeply sad can be considered as distinctly indicated by the tests, and if, in this doubt, we appeal to the words for which it was written (assuming this, though Händel often adapted to new words his own and others' music), the element of dejection is negatived, for it is grief and pity only that are their burden.

As to how it is that the phrases selected come to have their sad expressiveness, some interesting judgments are given: (by E, F, J, K, M, N and *E*). The principal consensus to be detected among them is the recognition by J, M (sighing) and *E*? (hopeless suggestion) of a likeness to the intonations of the voice in the expression of sorrow, exhibited by the progressions of thirds (*b*, *c*, *e*, *f*), which suggested bodily movements to Gurney; F also remarks upon this likeness, but asks whether it may not mainly be due to the portamento of the violin (wailing). The image of speech interrupted by expressions of grief seems to be prominent throughout both to M and to *E*, the sad expressiveness of

phrase *i* being concerned, perhaps, to the former with downward vocal movement. While the same phrase suggests giving way to E and to J, this is undetermined as either vocal or muscular. To I and N, on the other hand, phrase *i* carries a certain consolation with it. N's picture and that of K are drawn throughout in lighter colors than those of M and E; M finds only sad words and *E* bitter conclusions in the sounds that to N have consolation, and to K grandeur in them. In the latter image the melody is no longer a voice, but a life, and the interruptions of the accompaniment not sights, but the fates that come between it and its aims. But although K seems to have had no suggestion of sinking voice or drooping limbs, he mentions other elements of melancholy in the air besides its suggestion of thwarted human plans. Like M he remarks upon the introduction of three flatted notes, d, c and g. The first (occurring in *e*) acts through changing the key of the melody from e*b* to a*b*. Why this change should have pathos does not at once appear. Certainly, on the face of it there is no necessary suggestion here of downward movement; we cannot say that a*b* is lower, or higher than e*b*; it depends on what a*b* and e*b* we choose. There is, perhaps, a more recondite factor here at work, that of the interdependence of the keys of the modern European musical system. According to the theory of Hauptmann of keys a fifth apart, as those of e*b* and a*b* may be conceived to be, the lower is a relaxed form of the higher; a key *strives* into that of its upper fifth and *sinks* into that of its lower fifth. Into this point we cannot go further. The second two flatted notes, g and c, change the *mode* of the melody to minor; and this may be admitted as a sufficient reason for an effect of sadness, although why it should be so is, as above noted, not yet distinctly made out. We can add a third way in which all three of these flatted notes may have aided in the effect of this music; through their suggestion, that is, of a failure to attain an aimed-at height. For all these notes are slightly lower (by a semi-tone) than others which still linger in our minds from the earlier parts of the fragment (d, g and c natural), and the striking of this slightly lower point of pitch the mind may interpret as failure, or as a sign of diminishing strength.

Summing up, the test indicates that the burden of the fragment is perhaps deep sadness rather than dejection. As to the principal factors in the effect Gurney's opinion is confirmed, viz., descending thirds: g*b* in h (our replies mention also the other flatted notes); but the former seem to have acted upon our listeners rather through vocal suggestions than through images of bodily movement.

VIII.

The Bach Prelude, in which Rubinstein finds so remarkable a tragic expressiveness, suggested this word to one and only one (C) of our listeners. Two (I and O) acknowledged it in indirect tragic import by calling it funereal. While this result certainly bears against Rubinstein's interpretation, it suggests searching among the other replies for some general content of which the tragic may be conceived to be a special form. We shall, I think, find in them a hint of such a content; but an outline of import which a look at the musical make of the Prelude will once more convince us is nothing extraneous to the composition, but simply the combination of two characteristics of the mass of auditory perception of which it consists. Certain of the replies indicate, moreover, another than a tragic picture of which these characters form the outlines also.

The only consensus which is at all striking among the replies is the agreement of a few listeners upon what may be called a certain fragile inconsequence about the music. L uses the word incompleteness, and further hits upon the technical character of the piece in calling it an introduction; it is simply unsatisfying to B; to J unstable; to M disjointed; to K perhaps this, but better whimsical, and this latter judgment is repeated in the impression of D, where the element of want of connection appears in the idea of improvisation (playing and dreaming) and that of incongruity in the adjective fanciful.

Looking in the music to see whence this impression originates, we find (and this we shall have to content ourselves with simply claiming) that the quick moving melodic element of the texture awakens naturally in the ear of the listener anticipations of its further course that in the event fail of realization. We find, in other words, that inconsequence is a characteristic of a certain factor in the music; this factor is, moreover, a *light* complex of sensation, by which is meant only that it lacks intensity and volume.

But contrasted with this melodic inconsistency and incompleteness we find in the piece massive harmonic complexes (chords) in regular and often undisturbed recurrence. And looking back among the replies we find several which may be interpreted as recognitions of this element in the texture. *C* finds the music heavily monotonous; to *A* it is at least satisfactory; *B* finds it soothing, and to *K* it expresses contentment.

In the reply of *E*, finally both elements are recognized, heavy uniformity and delicate waywardness. *E* is the only

auditor familiar with the piece, and gives in her reply what we may claim as another confirmation (besides that of C in I. and *I* in VI.) of the remark in the invitation to the experiment that "an interest in the purely musical aspect of a composition might hinder rather than help" its imaginative interpretation. For usually so full of fancies drawn from life, *E* records here only a structural image taken from (what is called) the nearest art. The "delicate tracery of the frescoing and pillar ornamentation" in this reply suggests Hanslick's phrase, "the many daintily elaborated salt-cellars and silver candlesticks of the revered Sebastian Bach." Yet *E* recognizes first in the music "the massiveness of a cathedral."

The replies that remain record principally emotional as distinguished from intellectual impressions from the piece. It is gloomy to *F*; to H expresses sadness, and to *J* languor and reluctance; while dignified, it is non-emotional to *G*; it is sad although elevated to *L*; A finds it religious; to *H* it is philosophically elevated and to a dizzy height; while N hears in it "a seraph's song, a song as of one excelling in knowledge."

The grave character that all these listeners recognize in the piece is certainly in part the shadow cast by its minor mode, according to the mysterious habitude of this musical form. But most find also an elevation in the music, and this agreement points, I think, to another imaginative picture besides that of the tragic which can be drawn within the outlines laid down by Bach in the texture of sound he created. Which of these can be called the burden of the piece? If either can be our evidence is insufficient to decide.

We have found in the music two strongly contrasted elements: massive complexes of tone in continual recurrence, and a light current of melody having a certain character of inconsequence. It is easy for the fancy to weave between these two presentations of the sense a relation of cause and effect; to make the light inconsequence the result of the heavy insistence, and to picture further the strong monotonous chords as some unswerving natural force or some changeless divinity, and the wavering and often tremulous melody as some personality powerless in comparison. If now the music be approached in a troubled temper and its gravity be heard as gloom or sorrow, there is no thought more natural than that of the wreck of human plans by some over-ruling power. This is the conception of the tragic: the frustration of human desire by some remorseless fate or by the immutable decrees of some divinity; and its mood of feeling is that into which the melancholy of the music transmutes itself at these thoughts—awe before one and compassion for the

other—the fear and the pity of tragedy that Aristotle tells us purify the soul. But if these same melodies and harmonies be heard in a serene or buoyant mood, though the same picture be before us of created weakness in the hands of sovereign power, we see no more a life whose ruin tells of the terrors of divinity, we hear a trembling voice dying into silence before its glory; there is that within us that cast out fear, yet awe remains, and aspiration toward that seraph state.

IX.

In the Don Giovanni serenade the voice melody and that of the mandolin very naturally suggest two contrasted forms of personal mood. The mandolin music brings to the mind a mood of activity of a petty emotional content, the music of the voice a mood of passion (desire) of considerable emotional content. These are mutually exclusive conditions of the soul, and combined in one nature either must be assigned to different strata of it or must be conceived to dispute with one another its possession. That is, a nature cannot at one and the same time be in a predominantly active temper of trifling emotional excitement, and in a predominantly passive frame of fervid feeling. These must alternately occupy the spirit, in which case one may be more frequent or longer sustained or nearer to the personality in being concerned with ranges of idea more intimate in its life, or again there may be nothing to choose between them in these respects. The former supposition may be symbolized as superficial and deeper strata of a nature; in the latter case the personality becomes to this extent what we call a contradictory one.

It seems to me that this serenade makes plain to the attentive beholder what the character of a Don Juan really is: a nature of which levity is the controlling note, notwithstanding numberless fits of amorous gravity. His passion is then superficial, there is a hollowness, a deceit about it. It lies very near to ascribe its expression at all to a *wish* to deceive, to find in the levity a mockery; at least when guided by the operatic situation, one's imagination easily takes this step.

But certainly on the evidence of our replies, the Don Juan character cannot be claimed as the burden of this piece, considered simply as a structure of tone. The effects of *song*, it is to be remembered, are no longer those of sound alone. A song must have words and there must be some one to sing them, and these additions, of course, present us with a more or less definitely outlined character ready made. Such effects do not come within the scope of our inquiry, which is concerned with the expressiveness of structure of tone alone. It

is true that when asked for contrasted personal traits, which these two melodies suggest, our auditors agreed upon a certain earnestness in the voice part and a certain gaiety or indifference, or both, in the mandolin accompaniment. But some picture a character in which the earnestness is fundamental and the gaiety superficial; with some the gaiety is the undercurrent, and the seriousness the outward appearance; and with others they are simply contrasted moods. And whatever our conception of a Don Juan nature, it can hardly be that the picture is indefinite on a point as fundamental as this. Indeed, it may be said that there is no more agreement among these replies than was put into them in advance by the question asked, plus their recognition of the main contrast of emotional character between the sound complexes in question.

The lighter elemental is fundamental (as we have supposed it in Don Juan) in but five replies at most: so J (earnest character, with undercurrent of joyousness); L (merry temperament with serious intent); D (childish interest in a fiction); and especially in the replies of F and A, the only listeners to whom the music expressed the personal type we have assumed. F finds it only in the accompaniment, which depicts "a rollicking roué," and recognizes "more depth of character" in the air; *A*, who, though familiar with, did not recall the song at the time, detects in it exactly the Don Juan attributes of "levity and amorous sentiment."

Four of the replies do not specify either element as predominant; but describe contradictory or at least mixed moods: so H (passionate plea in a laughing accompaniment); I ? (mountaineer); K (Marie Bashkirtseff); *F* (womanly contradictions).

In the remaining seven the earnestness is made the underlying element, and frivolity the superficial one: so B (higher purpose triumphing over the lower); K (David Rizzio); M (worldliness with undercurrent of sincerity); *C* (frivolity concealing real earnestness); *G* (womanly seriousness beneath a gay exterior); *E* (frivolity with steadying strain of seriousness); *K* (playfulness feigned to conceal a sad heart). These last two replies, since the piece was known to the listeners, seem to involve an opposite conception of the Don Juan character to that here assumed. The reply of O (feeling in air; villainy in accompaniment) can be quoted in favor of our own; but certainly neither one nor the other is in any way indicated as the burden of this music.

A sufficient reason for this difference of opinion is, it seems to me, to be found in the nearly equal musical importance of the two contrasted melodies of the Serenade. Even were one

given with the fullness of voice tones, and the other in the gossamer of the mandolin, we could as well imagine either that the singer was speaking his heart while his hands were weaving a deception, or that his members were really at war with one another, as that his hands were revealing what his tongue was trying to hide. Certainly, if this is so, any special Don Juan significance vanishes out of the music; it is Marie Bashkirtseff as well, half given to the world and half to art; or it is the unfortunate Rizzio and the echo of the minstrelsy that covered up his sighs. Indeed, taken out of its dramatic setting why might not this music tell us of some wayward girl that cannot listen to an amorous plea for laughing; or depict to us some other comedy of two characters?

X.

In order to get a new form of question, the test of the character of the Russian melody, "Der rothe Sarafan," which to me expresses a certain deeply sad resignation, was attempted indirectly by asking a judgment on the possible origin of the song. Of twenty-one listeners, by whom it was not recognized, three attributed it with more or less certainty to northern races, one, *K*, agreeing with me that it betrays its Russian origin through its "undercurrent of sadness," another, *C*, finding a "pathetic wildness" in it that recalls the Russian or Norwegian people, the third (L) giving no reason for his surmise "Slavonic." The first judgment of I (old English) was based on a resemblance in style to certain English ballad-music, but this listener writes me, since, that the thought also presented itself "How sad! were these people so oppressed that this was their secret life?" To this testimony may be added the note of *H*, that the melancholy of the song is characteristic; but, on the other hand, another listener (C) to whom the selection was also known, found no trace of the Russian character in it. On the whole there is no case to be made out for my view; especially since a larger consensus ascribes a German origin to the piece (so H, J, K, *L*? M, N, *A*, *E*, *F*, *J*); the spirit of sadness we associate with the Russian character certainly not being a Teutonic trait. Moreover, this Teutonic flavor is expressed as a certain simplicity only (H sentimentality). The impression on which the question was based is so strong that these negative results are interesting. It remains possible that earlier in the evening something more positive might have been the outcome of the test.

XI.

After the gay song in which the cards have foretold all manner of good fortune to Frasquita and Mercedes, Carmen deals them for herself: "Diamonds, Spades; Death? Do not lie! First I, then he; to both of us death!" The song "In van per evitar" then follows: "In vain to avoid their hard replies we shuffle them anew; it avails us nothing, the cards are sincere, they cannot cheat. If in the book on high the page is joyful, shuffle and deal them without fear; the cards will turn gladly in thy hands to announce thee pleasure. But if thou must die, if the terrible word is already written in heaven, the cards, to whose will thou needs must bend, will repeat: the tomb! Again; again; again; the tomb!"

According to the testimony of these words the song "In van per evitar" was the expression to Bizet of a mood in which there is felt: a capital desire (for life itself); a conviction of the absolute incompatibility of this desire with the decree of fate; and a feeling of submission to this decree. Trying to hear what the tones themselves say, nearly the same message comes to my mind; I find in them: a passionate desire for life; the certainty that death draws near; a complete resignation; the same elements without the conception of fate.

In estimating the amount of confirmation given this conception of the music by our replies, it is to be remembered that they were called out by what is distinctly a leading question. This determined that, in the minds of the listeners, the piece should be the vocal utterance of some special passionate situation. An outline picture was put before them—a human being speaking under stress of emotion—which, under the guidance of the music, they were asked to fill up as they would. It would only be by inference from our results that the independent expressiveness of this music could be reached; as they stand they indicate what it may be expected to accomplish, if certain outside aid be given.

The operatic situation is in its main outlines reflected in decided prevalent agreements among the replies. Seventeen out of twenty-four listeners agree that the singer is a woman; and eighteen agree that there is before her mind a picture of personal good fortune, and of some obstacle to its realization. But in Carmen's case this issue of personal fortune is (a) capital; the obstacle to the favorable alternative is (b) unconditional, consists, moreover, of (c) a decree of fate and calls forth (d) a feeling of submission. That the issue with which the song has to do is of capital importance to the singer is indicated, or can be inferred, in twelve of the replies: so *H*

(death); A, M, *L* (death of loved one); N (surrender of loved one); O, *B* (despair); *G* (bewailing past joys); C? D, *K* (unrequited love); *J* (passionate entreaty); to which may be added J (intense protestation); L (deep passion). The test then indicates that, conceived as a song, this music expresses a situation of supreme moment to the singer. One of the listeners (*H*) has even detected in it the ring of the death-song that Bizet had in mind to write.

The obstacle to the singer's happiness is represented or can be inferred as unconditional in ten of the replies: so A, M, *L*, *H*, D, F, *G*, N, *B*, O. It is not a lament but a plea to four listeners, E, G, *C*, *A*; the point being left undetermined by the remainder. We may take the agreement of this large minority upon an element contained in the words of the song to indicate hopelessness as, at least, a hypothetical element of its expressiveness. Possibly a similar remark is in place with regard to the element of submissiveness, which, however, can hardly be even read into more than seven replies: so A, D, E, N, O, *A*? *L*.

There is no agreement in regard to the character of the obstacle to the singer's happiness; it is death, the flight of time, an unloving heart, the demands of cause; but in no case fate. This alternative has indeed occurred to N, but to be expressly set aside. Since Carmen's trait of fatalism has its most conspicuous expression in this melancholy acknowledgment of the folly of a struggle against destiny, the discrepancy, in this point, between N's impression and what we must suppose to have been Bizet's mood, is interesting in view of his former coincidence with Mozart.

On the whole, our replies indicate the song to be a supreme lament, possibly hopeless and possibly submissive, at any rate with a certain calm about it. What is lamented or why it should be is not expressed.

Several replies include the whole of this content: so O (sublime, calm, despair); N, *H*, M, *L*, D and A (who adds another element); perhaps the reply of *E* should be counted among them.

Looking back over all our results we find the following mental content indicated as the probable or hypothetical burden of these eleven selections:

		Imagery.	*Emotion.*
I	Beethoven Prelude,	0	0
II	"O mio Fernando,"	0	Yearning.
	"Durch die Wälder,"	Vigor and light-heartedness.	

III	Sonata Pastorale,	Surrender.	Happiness.
IV	Ballade No. 2,	0	Peace succeeded by fear.
V	Beethoven Andante,		Religious sentiment.
VI	Barberina Aria,	Weakness and simplicity.	Pain and Desire.
VII	Händel Aria,	?	Sadness.
VIII	Bach Prelude,	0	Grave emotion.
IX	Don Giovanni Serenade,	?	Contrast of active and passive moods.
X	Rothe Sarafan,	?	?
XI	"In van per evitar,"	If a song: a woman's supreme, calm lament.	

While any assured opinion on so large a subject as that of musical expressiveness would be entirely out of place as the result of a single experimental test, any evidence, however meagre, gives some indication in regard to the subject to which it pertains. This indication, in the present case, may be admitted to go counter to widely held beliefs in regard to the extent of musical expressiveness. Our results point toward this conclusion in two ways. Although this programme is made up of specially expressive music, the amount of significance, imaginative or emotional, which can be made out from our replies is, it must be confessed, comparatively scanty. Further, they show us two ways in which listeners to music may easily be misled in regard to the amount of import in the tones they hear. In the first place, what is in fact a character of the given structure of tone may be mistaken for something external to it. Only the latter is what the music *means*, the former is what it *is*. The intensity or mildness, for instance, of a given note is not an element of its import or expressiveness; it is an attribute or quality of the sound. On the other hand, the height or lowness of a tone, using these words in their primitive sense, is not a character of a note, but an element of its import. A tone has no special place, whether high or low; but the extremes of pitch may be, and have been, combined with the determinations of place we call height and depth, and these imaginative elements then become elements of their expressiveness.

In the second place, what is in fact only a suggestion of a given structure of tone is mistaken for an element of significance in it. Because a music wakes a wealth of fancy and emotion in one's own spirit, one is apt to conclude that it must in the souls of others and in the same way. Our replies offer a good illustration of the untrustworthiness of this conclusion.

It may be worth while to call attention here to the fact that the present inquiry is a purely psychological one. The proximate aim of our experiment has been to find out what

it is that certain selections of music express. Its ultimate aim has been as stated in the invitation, to throw light on the question of the expressiveness of music in general. There is another question somewhat closely related to this and which is not always clearly distinguished from it. It may be formulated : does the *value* of music lie in the auditory impressions of which it consists, or in the movements of the spirit which accompany them? This is not a psychological question as to what the effects of music *are*, but an æsthetic question as to what they *ought to be*. Two different conceptions of the art are here set over against one another. According to one, music is an art of *emotion* (and fancy) in which tone is used as the means of excitement, only because it happens to be through the ear that the soul can be most deeply, powerfully, and variously stirred. According to the other it is an art of *tone*, an incorporation of beauty in combinations of notes, of which emotion and fancy happen to be the ordinary by-product, just as smoke is of fire. No settlement of fact determines a question of beauty any more than it does a question of right. A psychological investigation like that here entered upon, being an investigation of fact, no conclusions reached in it would be any contribution whatever toward the decision of this vexed point in æsthetics. They have a bearing upon it, nevertheless, in this way: in so far as they indicate that musical expressiveness has been overestimated, they indicate, too, that on the emotional theory of its nature the importance of the art has been overestimated also.

In regard to the method of an inquiry into musical expressiveness. That of the present test was devised in advance of any certain knowledge that such a comparative method was possible at all; in the event of further like experiment, two changes in it might be made.

One would be to do away with questions entirely, the music being left to make its own unaided impression. There were two motives which led to a relaxation in the present case from this norm of scientific directness. One was the feeling that without musical conundrums, as it were, to guess, the monotony of the evening would be intolerable and cause so much wandering of attention as to defeat our object. The other was the uncertainty as to whether, without external aids, music would have any expressiveness at all that was not too recondite to be put into words. Amid these doubts it was sought to hold a middle course between too much and too little verbal suggestion. But it seems now possible to say that with auditors skilful enough in the analysis of their own states of mind, and with both power and will to exactly report them, such an experiment, if not protracted too long,

needs no other foundation than the music itself. Our own made much too great a demand upon the mental and physical powers of the audience; a shorter programme is, therefore, to be recommended.

A second change of method is suggested by the fact that even among auditors of an approximately equal knowledge of music, and skill and interest in it, we seem to find differences in capacity to detect its expressiveness. It becomes of special interest to compare the impressions of those whose endowments in this direction are greatest. To this end, a preliminary test might be made in order to select an audience for the experiment itself. It is true our replies indicate that it may not always be easy to decide who these specially sensitive listeners are; but doubtless the testimony of the auditors themselves, as to the clearness and certainty of their impressions, may be something of a guide.

The present undertaking may be criticised in a variety of ways. It might, in advance, have been thought hopeless to expect to obtain by this method any semblance of the impressions that, under other circumstances, the same pieces might make upon the same listeners. But that the impressions here recorded are inadequate ones can hardly be claimed, now that we see with what detail they often reflect the musical make of the pieces to which they refer. Neither the presence of others, nor the obligation to have impressions, had the untoward effect on the susceptibilities of the listeners that we all feared. But it is possible that to some the whole inquiry may seem a search for something that does not exist. There are no prevalent agreements of spiritual state, it may be thought, among hearers of music; the expressiveness of structures of tone is an illusion. This assertion is, however, either a surmise or a prejudgment; and neither are valid against an attempt to bring evidence to bear upon a question. Or the assertion may be that the inquiry is at least off the track; the expressiveness of music being something other than our conception of prevalent agreements among its hearers. It is to be hoped that any who hold this opinion will advance the discussion of the subject by the presentation of some other theory of the meaning of the phrase. Or waiving this question, it may be contended that what music expresses is literally *unutterable*. We cannot learn from one another what it is; we must feel it for ourselves. Any attempt to discover it by a method of comparison, since we must proceed by a comparison of utterances, is foredoomed to failure. No interpretation, then, of music that was ever put into words but has entirely missed the true message of the piece—Beethoven's "Fate knocking at the door," Rubinstein's "Wind sweeping

over the churchyard," and all the rest. Although the world would be relieved of much nonsense, were this opinion general, in this extreme form it can hardly have many adherents. At least all that is most valuable, it may more plausibly be claimed, in musical expressiveness, is unutterable. Were our listeners never so sharp-sighted and never so deft with speech, all the best part of their impressions would be left behind in their hearts after the words had gone out of their mouths. Perhaps this is true. It is no news that there are realms of being beyond the reach of scientific inquiry; and among them may lie all that is best in the message of music. Our inquiry then can have but humble results; yet, how humble they must be we can never find out till we try.

Another criticism is unanswerable. It may be said that the opinions of these listeners are but a trifling contribution to a subject of immense extent, and that our conclusions are, accordingly, of the most problematical character. This they and I will at once admit. In itself the evidence we present is a small matter indeed; it becomes considerable only by comparison with that which has gone before of the same kind; for even a very small quantity is indefinitely greater than zero.

PSYCHOLOGICAL LITERATURE.

PSYCHIATRY.

William Noyes, M. D.,

McLean Asylum, Somerville, Mass.

Rouillard, *Deux cas de scarlatine compliquée de troubles mentaux*, Annales médico-psychologiques, 1891 XLIV. 262.

The author calls attention to the fact that while nervous and mental troubles are very frequent after small pox, less frequent after measles, they are very much less found after scarlatina. He does not affirm that scarlatina does not often attack the nervous system, for the relative frequency of the ataxic and ataxo-adynamic forms in scarlatina are shown by clinical experience. The connection of scarlatina with rheumatism is well known, as is also the fact that rheumatism plays a considerable rôle in nervous and mental diseases.

The interesting point to note is the late appearance of these troubles, after the eruption, during the convalescence, and even some time after the patient appears to have recovered,—a proof that the elimination of the scarlatinous poison takes place over a long time.

There are but few published cases in spite of the frequency of scarlatina.

Sée and Roger have shown the frequency of chorea after scarlatina. Genau, that of facial neuralgias, Sanné has observed a special rachialgia. Kennedy, in England, and Revillout, in France, have observed different paralyses, hemiplegias, paraplegias and monoplegias, generally curable and transitory. Weissemberg has published an observation on spinal meningitis in the course of the eruption, causing death in three days.

Mental troubles appear to have been observed more rarely. Shepard has published a case of paraplegia with transitory aphasia. In 1885 Abdy published one with partial hemiplegia and amnesia. Rouillard himself has reviewed the published cases of troubles of memory consecutive to scarlatina.

Rouillard adds to literature two cases of psychoses following scarlatina.

The cases presented no morbid heredity.

Case I. was that of a soldier aged 22. He had had three attacks of articular rheumatism.

During his scarlatina he had abundant albuminuria, with marked cerebral symptoms. After two months in the hospital he was sent home for convalescence, still very weak, but without fever. Three days after his arrival, without apparent cause, he had an attack of religious delirium, saying that he was no Christian, that he was a Jew, etc. That evening he had an attack of acute mania, uttering harsh and inarticulate cries, and seeing everywhere people who wished to kill him. The attack was so violent that he was covered with perspiration. A

viscid and sticky liquid came from his mouth. There was a second attack the next morning, with a high fever. At five o'clock that evening a third attack, with hallucinations, but the cries were less strong and the movements less violent. A period of depression succeeded this maniacal excitement, a melancholia characterized by an almost absolute mutism. To all questions the patient replied by shaking his head. In the course of this period the face was congested, the pupils moderately dilated, the respiration slow. The sensibility was preserved, but the perception suffered a marked retardation for the lower limbs. Constipation was obstinate. In spite of the apparent mental depression the strength was as well preserved in the lower as in the upper limbs.

If a limb was lifted the patient voluntarily kept it suspended until it was replaced in a condition of repose. During this whole period, from May to November, the patient presented no sign of any rheumatic pain. During August he slowly improved, and although he persisted in his mutism, yet his depression was not so profound. In reply to questions he would answer by signs with his head, and he appeared to wish to speak. The scarlatina had begun Feb. 24; on Nov. 2 his father announced that his son had recovered, and Rouillard was able to verify this. Patient replied intelligently to questions and had a perfect recollection of all that had happened. He had known what was asked him, but could not explain his inability to speak.

The second case was also a soldier, with no other heredity than rheumatism in the father. He had entered the hospital for pneumonia. On the sixteenth day, while convalescing from the pneumonia, an attack of scarlatina came on. This assumed a grave character from the beginning, and was complicated in a few days by a pleurisy. The urine contained a marked amount of albumen, and the patient fell into a state of stupor and enfeeblement with subnormal temperature.

Two months after admission he was sent off for two months of convalescence. At the end of the first month he had an attack of epilepsy. On his return to his troop two months later he had another, and this was followed by others.

In the discussion Auguste Voisin recalled two cases of acute mania in two girls in the first period of scarlatina.

Charpentier considered that Rouillard had not proved the existence of scarlatina, referring to the fact that among the insane there may be erythemata simulating scarlatina.

Gilbert Ballet also took exception to the conclusions of Rouillard, who seemed to consider that he had described a special form of epilepsy and insanity—a scarlatinous epilepsy and insanity. With regard to the first case he would not agree with Rouillard. He would be inclined to think that the epileptiform attacks were symptomatic of uræmia, although in certain details Rouillard's observation seemed to contradict this interpretation. Even admitting that they were veritable epileptic attacks, this would be by no means a reason for designating the trouble a scarlatinous epilepsy. He would hold further that it had not been proved that scarlatina played any rôle in the development of the mental troubles described by Rouillard. In reply Rouillard claimed that he had broached no theory, but had simply reported two cases, which were rare, and well authenticated; he had not spoken of scarlatinous insanity, but had only spoken of mental alienations developed subsequent to scarlatina; there was no scarlatinous insanity any more than there was a cardiac or a cancerous insanity. But it is still necessary to recognize that etiology is the principal factor in the classification of mental diseases. Without saying *post hoc ergo propter hoc* it is necessary, however, to take into account the conditions under which insanity develops. Is there an insanity consecutive to acute diseases, and

especially to eruptive fevers? And does this mental affection have special characteristics? Rouillard does not know, and does not wish to prejudge the question. Observations must be collected and facts grouped in order to be able to draw useful conclusions.

Auguste Voisin recalled two cases of Bright's disease, in the course of which there had been several epileptiform vertigos, followed by hallucinations; by melancholic delirium, characterized especially by the idea that they were not at home, not recognizing their furniture, nor their pictures, nor their rooms.

THAYER, *A case of melancholia following typhoid fever*, Johns Hopkins Hospital Bulletin, 1892 III. 12.

Twelve days after the patient's temperature had become normal in his convalescence, he appeared for the first time to be nervous and anxious about his condition. Asked if he was very ill and if there was any chance of his recovery, saying one of the patients had told him he was very ill. Fifteen days after the temperature had become normal, his physical condition having steadily improved, and the diet having been increased to nearly normal proportions, he was allowed to sit up for a short time out of bed. On the same evening he was found to be in a very nervous condition. He was despondent, weeping, and when the physician came by, seized his hand and begged him to save him. He declared that he had seen the head nurse read the order that he was to be cremated that evening, and had also seen her receive the announcement that the box in which the operation was to be performed had arrived and was stored in the room below. The patient became more silent and despondent, and was discharged two weeks later in a distinctly melancholic condition.

Dr. Hurd, in the discussion, said that such cases of insanity after any exhausting bodily disease were not uncommon. Insanity developed after the eruptive fevers, pneumonia, acute tuberculosis, typhoid fever, and in fact any sequel which interfered with the nutrition, assimilation and blood-making power of the patient. The delusions of such patients were those which accompany innutrition, and were characterized by suspicion and apprehension. The deficient blood supply to the brain, or rather the altered quality of the blood supply by reason of the preceding bodily disease, explained the delusions. The development of post-febrile insanity generally points to an hereditary tendency to mental disease. The presence of this insane heredity developing active disease under such circumstances lends an unfavorable prognosis in most cases. Sometimes they recover, but the majority develop chronic forms of insanity.

Dr. Osler said that in his experience, especially after typhoid fever, the prognosis was favorable, and cited several cases reported by him in Fasciculus I. of the Johns Hopkins Hospital Reports for 1890, where apparent recovery from mental disease after a tedious convalescence occurred.

MAIRET ET BOSC, *Recherches sur les causes de la toxicité de l'urine normale*, Arch. de physiol. norm. et path. 1891 III. 273.

A reference to the experiments of these authors on normal urine will be of service in considering the results obtained from the urine of the insane. Rabbits and dogs were used in the experiments. Injections were made in the femoral vein of the dog and the auricular vein of the rabbit. Fresh urine from persons 22 to 33 years of age was used, the individuals being of almost the same body weight and living in the same average conditions. With regard to the degree of toxicity, it requires 100 cc. of urine per kilogramme of body weight to kill a dog. With this dose the animal succumbs immediately or after some hours.

Below this amount the animal may be more or less fatigued, but he does not die. In the rabbit 90 cc. cannot be injected without producing immediate death, but this may come on with smaller doses, between 70 and 80 cc. per kilogramme of body weight. In doses below 100 cc. the dog survives, but with the rabbit, below 70 cc., and even at 45 cc., the animal may survive or not, showing besides an immediate toxicity a remote toxicity. If these two kinds of toxicity are considered, it is seen that in the rabbit the degree of toxicity oscillates between 45 and 90 cc. per kilogramme of body weight, being on the average 67 cc. The effects of the urine were in general the same in the dog and in the rabbit. Taking the rabbit as a type, it is found that in all doses, even when the amount injected does not exceed 25 cc. per kilogramme of body weight, myosis is produced, but in small doses the myosis is but little marked, and is transitory; with larger doses it is more persistent, and the pupil becomes punctiform. With all doses there is abundant and clear micturition, which may be equivalent in quantity to the amount injected. The animal also shows thirst. Respiration is slowed. There is constant subnormal temperature, even to 3° or 4° C. Troubles of the nervous system consist, according to the doses, in somnolence, coma and epileptiform attacks, preceded or not by convulsive movements.

In studying the different constituents of the urine, to find the cause of the toxicity the authors conclude that it is the coloring matters that exert the chief toxic effect. To the objection that the coloring matters include at the same time some alkaloids, the authors cite the experiments of Pouchet and of Bouchard, to the effect that these substances are present in very feeble quantity in the urine, and Bouchard states that they have no influence on the toxicity of the urine. As regards the ptomaines, Æscher and Corninck state that they did not find these in normal urine. The coloring matters are, therefore, the essential cause of the toxicity of the urine. The salts of potassium also aid in the toxic effect, while the urea, the salts of sodium and potassium, and the water, have an effect on the micturition, while the salts and the water aid in the respiratory troubles, and the salts of sodium in the circulatory troubles.

MAIRET ET BOSC, *Alienation mentale par troubles de la nutrition, preuves expérimentales de l'existence de ce genre d'alienation*, Annales medico-psychologiques 1892.

In attempting to ascertain the genesis of certain cases of mental alienation and to study their causes, it is found that these cases are developed subsequent to some grave physical disease, such as typhoid fever, or during the puerperal state, or again at certain periods of the evolution of life, as at puberty.

In these cases it is natural to connect the insanity with these physical perturbations, and this with all the more reason since no other cause is found susceptible of explaining the mental alienation, and that the form and evolution followed by this mental disturbance have special features. The majority of clinicians admit this subordination. Yet the opinion has its adversaries, and is lacking in scientific proof. The authors claim that they have furnished this proof by experiments made of the toxicity of the urine of the insane. In this connection they have studied successively the toxicity of the urine of patients affected with mania, stupor, melancholia, the insanity of persecution, and senile dementia. As much as possible of the 24 hours' urine of these patients was collected, and from this was taken the quantity necessary for the experiments. As subjects of experiment, the dog and rabbit were used, more particularly the dog, whose nervous system is more developed, and whose reaction is more sensitive and more complete than that of the rabbit. The intravenous method was used, according to the rules for-

mulated by the authors in their researches on normal urine (Comptes rendus de la Société de Biologie, Dec. 13, 1890, and in the Archives de Physiologie, April, 1891). The results obtained with normal urine served for comparison of the effect of pathological urine, the results being tabulated for comparison. The comparison was made from two points of view, from that of the degree of toxicity and from that of the toxic qualities, i. e., of the action of the urine on the different functions of the economy, digestive tract, respiration, circulation, temperature, pupils and nervous system. In the first part they analyze the principal results obtained for each form of mental alienation, referring for details to a work they are about to publish on the toxicity of normal and pathological urine. In the second part they compare the results with each other in making the synthesis. They claim that this synthesis furnishes the scientific proof that the physical perturbations that they describe may give rise to the mental alienation, and they establish the nature of this alienation. The experiments on the urine of maniacal patients were conducted on thirty-three patients, twenty-five of the agitated type, eight being quiet. In mania without agitation five experiments were made on rabbits and three on dogs. In the two cases the results had a great resemblance to those for normal urine. The urine of non-agitated maniacal patients was, perhaps, a little more energetic in the sense that in the dog for example, myosis was produced by doses which normal urine did not produce, and the enfeeblement was more marked.

In mania with agitation the results are divided into two groups, the division being independent of the agitation. From the first group of ten patients the urine of three patients was injected into rabbits, and of seven into dogs. The results were in general the same in the two species of animals.

In comparison with normal urine the following facts came out: (1) As regards the degree of toxicity. The urine of agitated maniacal patients is considerably more toxic than normal urine. In the rabbit with normal urine death was never produced with less than 45 cc. per kilogramme of body weight, while with the urine of agitated maniacs, death followed twice with 25 cc. Again, in the dog it took 100 cc. of normal urine to kill the animal, while 30 cc., and even 25 cc., sufficed when the agitation was considerable. (2) As regards the characteristics. In general these are the same for myosis, micturition, respiration, circulation, temperature, nervous system, the manner of death, and the pathological findings. There was a little less hyper-excitability, and a slight hyperæsthesia at the upper part of the limbs. The urine of this first group of maniacal patients showed but little difference from normal urine, except in its greater toxicity.

The second group consisted of fifteen experiments on the urine of four patients, in whom the agitation was considerable, but not surpassing that of the patients of the first group, of whom four, in particular, were in a state of excessive agitation. Compared with normal urine this second group showed a considerable increase of toxicity, 25 cc. sufficing to kill the rabbit at once.

With regard to the characters of the toxicity the action of the pupil was more intense than with normal urine. Urinations were less frequent. Respirations were affected in the same manner, but the convulsive troubles are still more marked. The circulation is disturbed in the same manner. Body temperature is in general subject to the same modifications, i. e., there is a subnormal temperature, but the fact is less constant; in some cases, on the contrary, there is a rise of temperature.

On the side of the nervous system, together with similar symptoms, among which is weakness, the following differences are to be noted:

1. A high degree of convulsibility. The attacks, even in the dog, succeed each other rapidly, the slightest touch on any part of the body of the animal producing convulsive seizures, which may, in certain cases, become generalized.

2. An enormous muscular and tendinous hyperexcitability, and a very marked hyperæsthesia. Comparing the effects of the urine of the second group with that of the urine of the first group, it is found, with regard to the degree of toxicity, that these two kinds of urine resemble each other, while with regard to the toxic qualities there exist between them the same differences that were found in normal urine. Further, while in the first group the toxicity of the urine is notably diminished, disappearing even when a quiet period comes on; in the second group this toxicity may continue again very marked and with the ordinary characteristics, even when the patients are calm.

3. Stupor. The experiments are here divided into two groups relative to their toxicity; the first group is formed by cases of simple stupor, the second by cases of melancholic stupor. In simple stupor the degree of toxicity is a little more marked than normal; 60 cc. per kilogramme of body weight are sufficient to produce death, where it required 100 cc. of normal urine. The toxic qualities are in a general manner similar to that of normal urine. The sole difference, apart from the temperature, which may be affected in another manner, consists in a muscular hyperexcitability. In melancholic stupor nine experiments were made, six upon dogs, three upon rabbits. With regard to the degree of toxicity this is considerably increased; 25 cc. sufficed to kill a dog. The toxic qualities are the same for the other functions of the body except the nervous system, but here there are other symptoms manifested by inquietude and by stupor, resembling the condition of the patients who have furnished the urine. The action of the urine of melancholiacs compared with that of normal urine shows both resemblances and dissimilarities. The dissimilarities are with regard to temperature, which varies in both directions more than with normal urine; secondly, a diminution or even abolition of the sensibility, not occurring with normal urine; thirdly, psychomotor troubles; fourthly, the degree of toxicity of the urine of melancholia is considerably greater than with normal urine, 30 cc. and even 25 cc. sufficing to produce death. The greater toxicity and these different qualities have no relation with the density. In the insanity of persecution, when the individual is not agitated the toxic qualities are the same as in normal urine, but when the individual is agitated the toxic qualities are a little greater. The urine of senile dementia presents no special characteristics.

The following are the conclusions with regard to the degree of toxicity: In all the forms of mental alienation except senile dementia, the degree of toxicity of the urine is increased, but in very different proportions, according to the form of alienation and according to the acuteness of the disease. While in mania without agitation there is a degree of toxicity similar to that of normal urine, and in simple stupor and the insanity of persecution there is a relatively feeble degree of toxicity, melancholic stupor, melancholia, and mania with agitation on the contrary have a much greater toxicity. The form of the disease seems to be only a secondary element in the toxicity of the urine, the primary element being the intensity of the disease. In mania when the patient is agitated the toxicity of the urine is considerable; when he is calm his toxicity does not exceed the normal. With regard to the toxic qualities the experiments may be divided into two groups; in the one there are symptoms which do not exist in normal urine; in the other there are no new symptoms, but certain troubles which are produced by normal urine assuming an exaggerated importance.

In the second group there belong mania with agitation, simple stupor, and the insanity of persecution. In these cases, in fact, apart from a certain degree of muscular hyperexcitability and a slight amount of hyperæsthesia, the symptomatology is similar to that of normal urine, the enfeeblement and the prostration simply being more marked.

In the first group belong mania through nutritional troubles, melancholic stupor and melancholia. The cases of mania of this group show a hyperæsthesia, a muscular and auditory hyperexcitability, and a state of convulsibility that is not found with normal urine.

In cases where the pathological urine only reproduces the symptoms of intoxication by normal urine the degree of toxicity is closely connected to the greater or less intensity of the disease. In mania it is associated with the agitation, in stupor to the depression. When, on the contrary, the pathological urine gives rise to new symptoms, the toxicity persists in its principal characteristics, although the intensity of the disease has diminished, and only disappears when the individual has completely recovered his normal state. Consequently, while in the first case the toxicity of the urine is associated, not to the cause of the disease itself, but a secondary element—the acuteness, in the second case it is intimately associated with the disease itself. There is thus an essential difference between the diseases of the second group and those of the first. Studying these two groups from the etiological point of view, the causes may be divided into two groups: the ordinary causes of the neuroses, and secondly, such causes as infectious diseases, the puerperal state, puberty, etc.

Associating these etiological facts with those obtained from experimentation, it is seen that those cases which have as causes the ordinary causes of the neuroses, are those in which the toxicity of the urine is allied to a secondary element, agitation or depression, while those which are caused by a profound mental trouble are the cases in which the toxicity of the urine appears to be allied to the cause of the disease itself. Experimentation, therefore, appears to confirm that which is indicated by the study of the causes, and to show the existence, in addition to the neurotic mental alienations, of mental affections of another character. Experiments, however, do not warrant us in going farther and determining the exact nature of these mental alienations. It is of little consequence what physical cause has given rise to the insanity, the puerperal state or infectious diseases, the urines always produce the same toxic phenomena. Consequently, it is not to the typhoid or puerperal poison, for example, that it is necessary to assign the mental alienation, but to some other element common to all these perturbations, and as the sole common element is the trouble brought on by nutritive disturbance, we are forced to assign these mental alienations to this trouble. Consequently, in addition to nervous mental affections, a place should be assigned to *mental affections through troubles of nutrition*. The experiments, therefore, appear to the authors to have a double interest. (1) They confirm scientifically the opinion that certain physical disturbances may give rise to mental alienation. (2) They unite in the same group, *mental alienation through troubles of nutrition*, the mental alienations studied by authors under the different names which have been the causes capable of producing them: puerperal insanity, insanity of pubescence, etc.

FOLSOM, *Some points regarding general paralysis*, Boston Med. and Surg. Journal, Sept. 3, 1891.

Of the many divisions of general paralysis into several clinical types, all of them naturally more or less arbitrary, Folsom considers Meynert's

eight the most satisfactory *(Klinische Vorlesungen uber Psychiatrie, Wien, 1890, Braumüller)*.

(1) Simple progressive dementia, with the usual progressive motor impairment which accompanies it.

(2) With delusions of grandeur and with marked motor disturbances, which appear simultaneously and are progressive. The mental state is usually of exaltation, but there may be depression.

(3) Of the same type as the last, but failing its steadily progressive character, that is, with remissions.

(4) Cases in which the characteristic exaltation and grand delusions reach such an astonishing height that the manifest motor symptoms are looked for with confidence from day to day, and yet may not appear even for a year, any slight incoördination naturally being obscured by the general muscular disturbance. Meanwhile there may be such an improvement as to simulate a recovery.

(5) A very rare form with alternate symptoms of exaltation and depression.

(6) With early furious delirium, painful hallucinations, confusion and incoherence somewhat resembling acute delirium.

(7) In which the characteristic indications appear secondary to other forms of insanity, for instance, after paranoia or melancholia.

(8) The combined form, with sclerosis in the whole cerebro-spinal tract, the symptoms of tabes or spastic paralysis predominating, according as the posterior or lateral columns of the cord are chiefly involved. The ascending type, in which the cord is first affected, is rare. Optic neuritis, ending in atrophy and paralysis, especially of the ocular muscles, may precede marked mental symptoms.

In Paris in 1874, and chiefly by Sander in Berlin in 1876, attention was called to a period in general paralysis in which there are vague signs of mental failure for a varying length of time, perhaps for several years antedating the pronounced symptoms. This early stage is most marked in Meynert's first class, the demented type, to which the recent great increase in general paralysis belongs.

Of the different pathological designations of the disease, chronic meningo-encephalitis, chronic diffuse periencephalitis, Folsom prefers chronic diffuse cortical encephalitis, whether primarily interstitial or parenchymatous, ending in greater atrophy than occurs in any other form of insanity. For the microscopic changes found in the cortex the description of Mendel is quoted (reviewed in this Journal, III. p. 560).

Folsom calls attention to the fact that the post-specific cases, with a previous history of specific disease not recent, those not only not benefited by iodides and mercury, but usually debilitated and injured by them, may exhibit post-mortem the same microscopic changes as those in which there is no ascertained evidence of syphilis. In paralytic dementia, with a recent history of syphilis also, and with marked indications of specific disease, where anti-syphilitic remedies avail to produce such an amelioration of symptoms as to simulate a cure at least for a time, the same diffuse cortical changes may be found at the autopsy-points, which, in making and verifying diagnoses, should be borne in mind as well as the facts that there are degenerative changes in the brain secondary to gross syphilitic lesions, which do not constitute general paralysis, and that the several types of general paralysis and other conditions of cerebral atrophy exhibit post-mortem appearances which may so gradually shade off into each other as to make the analogy very close. In senile and chronic simple insanity, the atrophy of the nerve fibres is primary, while in paralytic dementia the essential process, according to Obersteiner, is a diffuse primary sclerosis of the cortex, which leads to atrophy, which appears in the frontal lobes first. Folsom follows Obersteiner's view that the sclerosis is preceded

by a condition of irritation which seems to justify the expression, chronic periencephalitis, although the brain-coverings play only a secondary *rôle*. Obersteiner's views that the "spider cells" are formed from wandering leucocytes, are quoted, together with his description of the processes leading to fibre-atrophy. Healthy and diseased cells and fibres being seen side by side, the early symptoms do not constitute a paralysis, but a cortical ataxia, a motor intelligence-disturbance on the one hand, and on the psychic side mental failure due to defective association of ideas through greater or less affection of the association-fibres of the cortex. Further than that we can scarcely yet be said to know much about the relations of pathological conditions to abnormal mental manifestations in general paralysis, except so far as the final atrophy explains the intellectual and physical decay. Folsom's description of the prodromal stage of general paralysis has already been reviewed in this Journal (Vol. III. p. 557).

CUYLITS, *Surmenage et folie paralytique*, Bulletin de la Societé de Medicine mentale de Belgique 1890 p. 271.

In this article the author attempts to make good his assertion that overwork, traumatism, the abuse of alcohol and tobacco produce no bad effect in a sane man. They may produce some form of mental alienation in a nervous hereditary subject; they may produce general paralysis when this hereditarily predisposed subject is syphilitic from birth or when he becomes so later. In assigning this specific origin to general paralysis he classes it with the diseases by intoxication of the same kind as the nervous accidents due to typhoid fever or diphtheria. The author urges that it is not easy to demonstrate by facts and arguments that overwork may of itself be able to cause general paralysis. He thinks that if a particular case is cited as a case of general paralysis from overwork that, unhappily for the demonstration, it would be difficult to establish the fact that he was not at the same time an hereditary subject, and on this account the observation would be without value. The contention of the author is that the normally endowed man cannot overwork, fatigue acting as a sort of safety valve, producing sleep in time to save the brain. If a man, apparently sound and healthy, with no sign of degeneration, overworks and becomes insane, he is *ipso facto* a degenerate, else he would not have broken down, and you have not looked deep enough for the signs of degeneration. Criticism is simply powerless before such an argument.

CHEVALIER, *La paralysie générale à l'asile de Dijon* (de 1843 à 1889), Thèse de Bordeaux 1889-1890 No. 52.

From a statistical study of the records of the Dijon Asylum, Chevalier concludes:

1. The number of general paralytics has risen during 30 years from 13% to 20% for the men, and from 5% to 7% for the women.
2. The proportion is four times greater among the men than among the women.
3. The number of married paralytics is double that of the unmarried.
4. The average age is 38 years for the men and 40 years for the women.
5. There were no paralytics under 21 years.
6. From 20 to 25 the proportion is 1-5%.
7. The laboring class furnished about 30% of the number of paralytics; the commercial and industrial classes 25%; the liberal professions 5% (Dijon is a public asylum).
8. Among 163 paralytics whose hereditary and personal antecedents were established, there were 1-5 with an alcoholic heredity; 1-5 with a congestive and insane heredity; 1-13 with a history of syphilis.

Comparing these numbers with those given in the thesis of Dr. Talon, who has given a report covering the same period of years for the Marseilles asylum, it is found that the number of women relative to the number of men is a little larger at Marseilles than at Dijon.

The average age at the time of entrance presents an inverse relation at Dijon and at Marseilles, where the figures are 45 years for men and 35 for women. The number for the professions was the same at Marseilles and at Dijon.

Chevalier's general conclusions contain nothing new, and are in accord with the ordinary views on the subject; his conclusions are if it has been shown that general paralysis occurs with increasing frequency in connection with the conditions of existence, from whence arises an over-exertion increasing from day to day, it still must be recognized that this progression is not so great as certain authors have affirmed. If it is well established that the maximum frequency of the appearance of general paralysis is between 35 and 45 years of age, as all alienists claim, it has been equally shown that general paralysis at the extreme limits of life is not an exception, and the limits of its appearance increase every day. With regard to the researches into etiological causes there were such complete *lacunæ* in certain cases and in others such obscurities, reticencies and false statements as to render any practical conclusions impossible.

BLACKBURN, *A study of nineteen cases of general paralysis of the insane.* Report of the Government Hospital for the Insane, Washington, 1891.

All the cases were males, fifteen white, four colored. With possibly one exception all were characteristic in symptoms and in the lesions found post mortem. The skull was thicker than the average in seven cases; it was noticeably dense in five; thinner than usual in five. Various degrees of asymmetry, usually very slight, were observed in at least twelve cases. The horizontal outlines of eighteen of the crania are shown in two plates. The dura mater was abnormally adherent to the bone in six cases; the inner surface showed evidences of internal pachymeningitis in six cases. The heaviest brain weighed $51\frac{1}{4}$ ounces; the lightest weighed 34 ounces; the average weight was $43\frac{2}{5}$ ounces.

Marked changes in the pia and more or less shrinkage of the convolutions were found in nearly all the cases. The meningeal and atrophic changes were usually more decided in the frontal portions of the hemispheres. In thirteen cases the pia showed adhesion to the cortex; in the remainder the membranes were removed with even less difficulty than from the normal brain. The microscopical appearances were characteristic in nearly every case. As a rule the microscopical changes were of greatest intensity in sections from the fronto-parietal convexity, though occasionally the hippocampal regions showed the most decided changes.

In the majority of cases slight vascular and other changes were found in the cerebellum. The pons and medulla were diseased in all the well-marked cases. Slight sclerosis of the spinal cord was found in several cases. The report is accompanied by four excellent photographs of paretic brains.

FROELICH, *Deux fractures spontanées chez un paralytique générale*, Revue méd. de l'est, 1890, XXII. 561.

The author cites the opinion of J. Christian in the *Dictionnaire des sciences médicales* against the alleged exaggerated tendency to fractures in general paralytics, Christian not having seen a single fracture in five years in 307 paralytics. Also, Simon in his thesis *Des Fractures Spontanées*, 1886, asserts that spontaneous fractures are very rare in general

paralysis, most authors mentioning them, but without offering any proof, there existing only a small number of observations that are truly conclusive, and most of the authors failing to distinguish, from the point of view of the production of these fractures, between dementia and general paralysis.

Froelich's case was that of a man of 43 years, who presented himself as an out patient, having a fracture of both bones of the fore-arm. Three days before on lifting a shovelful of dirt he felt a slight pain in his arm and at the same time heard a slight cracking, but he continued his work.

The clinical history as given by Froelich leaves no doubt that the patient was suffering from general paralysis. That the fracture was really spontaneous there seems to be no doubt, since shoveling dirt would not produce a fracture in a normal man. The manipulations necessary for putting the arm in a plaster dressing caused no pain to the patient. The patient returned five weeks later to have the plaster removed; union was complete. At the same time he showed his right arm, and examination showed that the two bones of the fore-arm were broken. He had slipped on a flight of stairs, and in falling struck with the back of hand, not very strongly, on some coal in a basket that he was assisting in carrying. The traumatism here was more considerable than in the first instance, but the shock was not violent and he did not know that he had broken any bones.

The points of interest are:

1. In a general paralytic, whose disease had existed over 6 months, two spontaneous fractures occurred at intervals of 5 weeks.

2. These fractures caused no pain to the patient at the moment of their production, nor at the time of their reduction.

3. The union was rapid, as has already been noted for this class of fractures.

BUDDEBERG, *Ueber die akut verlaufende depressive Form der Dementia paralytica*, Allg. Zt. f. Psychiatrie 1890 XLVI. 682.

Within a short period Buddeberg observed five cases of the depressive form of general paralysis. On entrance all three cases presented the classical picture of agitated melancholia; only in three cases was there a certain diminution of memory. Patients complained loudly of their misery, wrung their hands in despair, ran unsteadily about, and refused nourishment; hypochondriacal complaints were more rarely shown. After a short time there developed in all the cases signs of a profound organic lesion of the brain, as shown in difference and immobility of the pupils, pareses, etc. To the paretic symptoms there was added a rapid loss of mental powers, nutrition was greatly reduced, and on the entrance of intercurrent, febrile diseases, the disease ended fatally in the course of a few months; only one case lasted eleven months. Autopsies in four cases; the brains in general showed signs of a very acute encephalitis, the cortical substance being already in part atrophied. A cystoid degeneration of the cortical substance such as Schüle and Ripping have described for this form was not observed. As regards etiology four cases were hereditarily predisposed. Besides *trauma capitis*, mental over-exertion and work appeared to be important predisposing causes, but the number of cases is too limited to permit definite conclusions to be drawn. Patients all males.

COTTAM, *A case of general paralysis of the insane with crossed reflexes*, Lancet 1891 II. 288.

The patient was a male, age 55. The clinical history presented the usual mental and physical signs of general paralysis. The particular symptom of "crossed reflexes" was noticed after the disease had

advanced to the stage that compelled the patient to be kept continually in bed. At this time, in addition to the exaggerated knee-jerks, it was noticed that tapping of either patellar tendon was associated with a contraction over the outer and upper part of the opposite thigh in front. This crossing occurred with both knee-jerks; but on tapping the left patellar tendon, the contraction on the right thigh was more marked than that on the left thigh, which followed tapping of the right patellar tendon. That this was not due to what Ross calls the "physical diffusion of vibration" was, Cottam thinks, shown by the fact that the contractions could be plainly felt as well as seen, and also that the crossed contractions, which could almost always be elicited, occurred after the ordinary reflex. Again, the plantar reflexes were faint, but associated with the same phenomenon, the "crossed contractions" occurring in the same locality as in the case of those following the knee-jerks. "Front tap" contractions could be obtained, and these also brought out a precisely similar "crossed contraction." Ankle-clonus was faintly present, and there was not any crossing. Of the other reflexes, none of which showed any associated crossing, the inter-scapular, abdominal, and epigastric were absent, the gluteal brisk, cremasteric faint, and the pupillary sluggish. No autopsy could be obtained. Prevost, who has reported a similar case of crossed reflexes, regarded the crossing as due to the physical diffusion of the vibration, for he found that "section of all the nerves and posterior nerve roots of one limb of an animal does not abolish this crossed contraction." With regard to the two theories regarding the nature of the knee-jerk, Cottam considers that his case is an argument in favor of the theory that holds the knee-jerk "to depend on a centre in the spinal cord," as against the theory that the "contraction of the quadriceps is due to local irritation of the muscles from sudden elongation," and he asks how we are to account for the contraction in the opposite limb if the contraction depends on local muscular irritation. He denies the probability of any vibration, and thinks that the only diffusion that occurred was from the one to the other side of the spinal cord itself.

Garnier, *La folie à Paris, la progression cerrélative de la folie alcoolique et de la paralysie générale*, Annales d'hygiene publique et de médecine legale 1890 (3) XXIII. 5.

1. Statistics show that the number of the insane in Paris has increased in recent years in very strong proportions. the frequency of insanity having increased about 30% from 1872 to 1888.
2. Mental alienation is more common in men than in women (men 55.6%, women 38%.—General statistics of the préfeture de police).
3. The statistics of the increase of insanity for the two sexes in the triennial period, 1886-1888, is for men 59.35%; for women 40.64%.
4. Insanity considered as a whole and with regard to its monthly movement regularly attains its maximum frequency each year in June, and its manifestation or the development of the period of access, both among men as among women, seems to be favored by the influence of spring.
5. The increase of cases of mental disease in recent years is first of all shown in two types, whose frequency has increased very rapidly, alcoholic insanity and general paralysis.
6. The increase of alcoholic insanity is so rapid that its frequency is to-day twice as great as five years ago, and the commitments have increased 25% in the course of the last triennial period. It forms almost a third of the cases of mental diseases seen at the special infirmary.
7. Females have a proportional participation in this increase, and this participation tends to become greater and greater.
8. The frequency of alcoholic insanity is subject to strong monthly

variations. It does not reach its highest limit during the hottest months; its increase appears to correspond to the influence of spring, with the monthly maximum in June.

9. Observation of the delusional forms of alcoholism shows that the reactions that develop under its influence are becoming more violent from day to day and are accompanied by more attempts on the life of individuals, consequences that it is legitimate to attribute to the alcohols of commerce actually in use.

10. General paralysis, which is with alcoholic insanity the morbid form whose increase is the most rapid, comprised 12.27% of the total patients examined at the *dépôt*. In five years its frequency has more than doubled.

11. It tends to become proportionally more common among women than formerly; the relation which was five years ago, men 79.60%, women 21.39%, is to-day men, 71.17%, women 28.82%.

12. As with mental disease in general, so with alcoholic insanity, but still more than any other morbid form, the greatest number of admissions of general paralysis is in springtime. Its increased recrudescence takes place in May, and is very markedly vernal.

13. The comparison between the curves showing the simultaneous increase of alcoholic insanity and of general paralysis shows that their rapid progression is plainly correlative. In the close relationship of their course the etiological influence of alcoholism upon the development of diffuse interstitial encephalitis appears to be manifest.

DARRICARRIÈRE, *La paralysie générale dans l'armée*, Thèse de Paris, 1890 No. 61.

This thesis is a study of the statistics of general paralysis in the army during the 10 years from 1878 to 1888. To the question whether statistics carried out on all men between the ages of 35 and 55 in civil life and on soldiers of the same age—manifestly the only legitimate method of arriving at results—would be to the advantage of civil or military life, he is unable to give a satisfactory answer.

ACQUÉRIN, *Contribution à l'étude médico-legale de la paralysie générale*, Paris 1891.

In a pamphlet of 74 pages Dr. Acquerin discusses the medico-legal relations of general paralytics, especially in relation to the early or prodromal stage, which he calls, not without justice, the *période médico-legale*. As the discussions of the responsibility of paralytics and of pseudo-paralytics have special reference to the French *code pénal* and *code civil*, they have but little bearing on similar conditions arising under English and American laws. Examples are given of crimes and misdemeanors committed by general paralytics, and examples of the status of such patients in marriages, contracts, life insurance and wills.

ZACHER, *Ueber zwei Fälle von acuter Paralyse*, Allg. Zt. f. Psych. 1891 XLVIII. p. 188; Neurol. Centralbl. 1891 X. p. 68.

The author reports two cases of acutely progressing paralysis, in which the first, after a melancholic prodromal state, ran its course in less than four weeks; in the second the duration of the disease was about two and a half months. In both cases, besides a relatively slight change in the vessels and in the interstital tissue, there was a fairly extensive and high degree of fibre atrophy. From this the author concludes that there are cases of paralysis where the fibre atrophy is the primary process in the anatomical changes.

Hertz considered that the two cases must be classed as delirium acutum, and expressed a caution against the too great extension of the

field of general paralysis. To the question whether the two cases might not be considered as an acute infectious brain disease, the author thought that this was disproved by the long prodromal stage in one case and the failure of all evidences of infection at the examination of the internal organs. Also up to this time, as Fr. Schultze has pointed out, no fibre atrophy has been found in the brain in acute infectious diseases.

ROCQUES, *De l'alcoolisme et de la paralysie générale*, Thèse de Paris, 1891 No. 230.

For a number of years general paralysis and alcoholism have shown a progressively ascending scale in Paris. The curves of the two diseases show a parallel course. Authors are divided upon this question. Some (Foville, Garnier) think that alcoholism is the cause of this increase of general paralysis, while others (Lasègue, Ball, Christian and Ritti) on the contrary think that alcoholism is only an accompanying factor, a symptom of the initial period of general paralysis, during which the patient under a general excitement gives way to excess of drink. Rocques holds to this last opinion. When the alcohol is eliminated and the alcoholic delirium has disappeared, the general paralysis alone comes to observation and continues its slowly progressive course. There are a great many patients classed as alcoholics who should be classed as paralytics. This error in statistics shows the proportion of paralytics to be 20% of insane patients instead of 27% as it should be, and is the cause of a corresponding increase in the proportion of alcoholics. It is necessary to reserve a diagnosis at the outset, since the prognosis of alcoholism is often favorable, while that of general paralysis is fatal. The responsibility of the alcoholic is a subject of discussion, while that of the paralytic is fixed.

Although alcoholism and general paralysis increase with parallel steps in urban districts, such as the department of the Seine and that of the Rhone, and although they are both rare in agricultural regions such as Lozère, yet in certain alcoholic countries there is proof of the rarity of general paralysis. This is the case in Finisterre, one of the departments where alcoholism plays the greatest ravages, yet where general paralysis forms only 0.62% of the cases of mental disease. The same facts are observed in countries that are manifestly alcoholic, such as Ireland, Scotland, Sweden and Norway, and Canada. Alcoholism may lead at length to general paralysis, alcoholics may beget children predisposed later to general paralysis. When general paralysis develops in an alcoholic, it assumes a special form, pseudo-general paralysis (Westphal), which is distinguished by numerous characteristics and especially by the course of the disease. It may be cured, or it may relapse. True general paralysis recovers very exceptionally; remissions are observed, after which it continues. Pseudo-general paralysis may begin again.

REGIS, *Note sur le diagnostic différentiel de la lypémanie hypocondriaque et de la paralysie générale progressive*, Gazette médicale de Paris, 1890 (7) VII. 1,13.

Regis cites four cases in which there was difficulty in diagnosing between hypochondriacal melancholia and general paralysis. In his conclusions he gives the diagnostic points of different authors and then his own. The principal distinctive characteristics given by different authors are: 1. The hypochondriacal delusion of general paralysis has a particular stamp of absurdity, hebetude and incoherence. It appears suddenly, it is changeable and inconsistent. The patients do not argue and they speak without conviction, and they show but little zeal in complaining of their ills (Baillarger, Marcé, Voisin, Luys, etc).

The delusion of melancholia may be monotonous, but it does not present the same character of absurdity. The patient sees his disease, the reason, and the explanation, endeavors to convince, makes complaints, becomes angry with his contradictors.

2. In general paralysis the hypochondriacal delusion may be complicated at any moment with delusions of an ambitious nature. This is never the case in melancholia.

3. The hypochondriacal delusion of general paralysis is not favorably influenced by morphine, contrary to the case in melancholia (Voisin).

4. In general paralysis the subjects are not hereditarily predisposed. There have been no previous nervous disturbances (Mendel).

5. General paralysis, and consequently the hypochondriacal delusion accompanying it, come on between 35 and 45 years of age (Mendel, Mickle).

6. The examination of the organs is almost always negative in general paralysis (hypochondria *sine* materia), (Mendel).

7. In general paralysis there sometimes comes on from the beginning slight apoplectiform or epileptiform attacks, pupillary and spinal symptoms (Mickle).

8. Subsequently, the signs of dementia paralytica can be established.

9. In anxious melancholia the hypochondriacal delusion is accompanied by ideas of damnation and of possession, by analgesia, by a tendency to suicide, and to voluntary mutilations, and by the fear of not being able to die. The delusion of negation and of enormity develops, and of the doubling of the personality (Cotard, Séglas).

These distinctive characteristics are far from being sufficient in practice to give certainty. And it is among the most important cases, those dependent on the nature of the delusion and on heredity, that they have the least value, since they may be found in both forms of the disease. Regis adds the following as being of some possible service:

1. Melancholia with hypochondriacal delusion is observed especially at an advanced age from 45 to 60 years. It is encountered more frequently among women than among men, in the proportion of eight cases to twelve, contrary to what is found in general paralysis. It is, like every psychosis, more rare among syphilitics than general paralysis, since the existence of a previous syphilis constitutes a presumption in favor of general paralysis.

2. The hypochondriacal delusion of melancholia does not appear at the beginning of the attack, but a longer or shorter time afterwards, some months or some years. It is constantly consecutive to the ordinary delusion of melancholia, especially to the delusion of imaginary culpability, which is the type. It continues associated to the delusion, and joins itself logically with it. It is tenacious, fixed and persistent. It is rarely accompanied by hallucinations; while on the contrary the terrifying dreams, the fear of death, the refusal of food, the tendency to suicide are almost the rule.

The patient is subject to paroxysmal crises more or less acute. During many years the intellect remains intact, the memory precise; the lucidity more or less great, sometimes complete.

3. The examination of the viscera is habitually negative; there is stomachic and intestinal inertia, constipation, frequency of the pulse, palpitations and more rarely other functional troubles. Emaciation is rapid; a true cachexia sometimes supervenes.

4. Recovery is possible; nevertheless patients may end in suicide, marasmus or the chronic state. It is especially in these last cases that one observes Cotard's delusion of negation, which appears to be the terminal stage of this form of melancholia.

In conclusion, Regis says that the hypochondriacal delusion as a special characteristic of general paralysis may be encountered in the

same form in anxious melancholia; that the diagnosis in these cases may present real difficulties; that, to solve the problem, it is necessary to bear in mind all the clinical elements of distinction.

ROUSSET, *Du role de l'alcoolisme dans l'étiologie de la paralysie générale*, Bull. med. de Paris 1891 V. 743; Gaz. d. hôp. Paris 1891 XIV. 871. (Abstract in Centralbl. f. Nervenheilk. 1891 Oct.).

At the session of the Congress of French alienists at Lyons in August, 1891, Rousset gives an extended review of this subject and makes clear the present position of the question. After showing the difficulties of the investigation of mental diseases because of the uncertainties of the clinical data, he reviews the different historical phases of the relations between progressive paralysis and alcoholism, showing the differences in definition that have been held on this subject among clinicians.

In the second place he treats of the errors which have often occurred, since the alcoholic excesses, which are very frequent in the beginning of general paralysis, were often taken for the cause of the disease. Twenty-two personal observations illustrate the different clinical varieties of alcoholic general paralysis. The conclusions of the author are as follows:

1. The *rôle* of alcoholism in the etiology of progressive paralysis has at all times been the subject of numerous controversies. The views of the authors may be classed in four principal divisions.
2. Certain patients considered as alcoholic paralytics began in fact in alcoholic excesses, but after the beginning of the general paralysis, so that these excesses are to be considered as results not as causes of the disease. This condition of recently acquired alcoholism need not therefore be taken into account in the causation of the meningo-encephalitis.
3. The correlative advances of alcoholism and general paralysis ought not to mislead us to the conclusion that one of these diseases has been produced by the other. The geographical and ethnographical relations do not seem to speak for the importance of alcoholism in the causation of progressive paralysis.
4. Extracts from the reports of all the asylum directors of France clearly show that the views of clinicians on this subject are still very much divided.
5. It appears that alcoholism plays a smaller *rôle* in the etiology of general paralysis than that uncertain, often unknown and impalpable "something" that is found in all diseases, and which seems to be a necessary condition for the development of the meningo-encephalitis, namely predisposition, which, according to the individual, may be cerebral, rheumatic and nervous, or alcoholic. In some, not very frequent cases, chronic alcoholism may bring on general paralysis without this predisposition, since in brings on a process of connective tissue growth and brain sclerosis.

Magnan of Paris opened the discussion by a clinical and anatomical demonstration of chronic cerebral alcoholism and insisted on the importance not only of the individual but also of the organs for the localization of the alcoholic lesions. For him there exists a general paralysis, but not an alcoholic general paralysis. The patients designated by this last name may be divided into three groups: first, chronic alcoholics with cerebral lesions; second, true general paralytics, who have remissions in the first stages of the disease; and third, the hereditarily degenerated, who under the influence of alcohol show cerebral symptoms which simulate progressive paralysis.

Régis of Bordeaux, from his observations in Castel d'Andorte, thought that it must be assumed that alcohol played only a secondary *rôle* in the etiology of general paralysis in the upper and middle classes of the

district; on the contrary from this point of view hereditary predisposition and syphilis seemed to him to be of manifestly greater importance.

Marie and Bonnet expressed similiar views from personal statistical data, that especially alcohol more frequently than other poisons (lead, mercury, morphine), but in the same way as these, contributed to the development of the disease in the hereditarily affected.

Combemale referred to his observations made first at Montpellier and then at Lille on dogs that he had accustomed continually to alcohol drinking; between the fourth and eleventh months these animals manifested a series of characteristic physical and mental symptoms, and showed at the autopsy the same signs as progressive paralysis.

Christian disputed this similarity, and thought that in these cases there was probably an encephalitis which differed anatomically from the meningo-encephalitis of progressive paralysis.

MARIE, *Contribution à l'étude des troubles oculaires dans la paralysie générale*, Thèse de Paris 1890, No. 349.

The thesis of Dr. Marie contains nothing that is not already known about ocular troubles in general paralysis. His conclusions are that:

1. Ocular troubles (ophthalmoplegias, amauroses, etc.) are frequent in general paralysis. They have a special importance in that they may precede by several years the beginning of the affection.
2. These early troubles are transitory and incomplete in character.
3. Post-mortem examinations show that the initial diffusion of the lesion of general paralysis extends to the peripheral nervous system as well as to the portions of the cerebro-spinal axis.

Confusional Insanity.

In his review of Mental Diseases for 1891, in the Annual of the Universal Medical Sciences, Dr. Brush, in referring to the article by Connolly Norman on the subject of Confusional Insanity (see this JOURNAL, iv. p. 326), comments as follows on Wood's use of this term:

"It seems to us that Wood has somewhat overshot the mark in his attempt to simplify the nomenclature of insanity and its classification, and has incurred the risk of confusing distinct clinical forms of mental disorder. The term "stuporous insanity," for example, while applicable to a class of cases etiologically of the same origin, physical or mental exhaustion, disturbed nutrition or malnutrition, and auto-intoxication, conveys to the clinical alienist the idea of a class widely differing, in its clinical picture, from some of the cases he attempts to group together. The term "stuporous," while it describes an apparent state, is, we think, an unfortunate one. The majority of the cases are not stupid, but, on the contrary, alert and watchful. In some an overwhelming delusion of terror dominates, as it were, the patient, and prevents all attempts at spontaneity. In others, the impressions are normally received and interpreted, but response cannot be evoked. The patient is in some sense mentally paralyzed, but he is not stupid. We doubt not, indeed we know from observation, that cases which have been classed under the head of "confusional insanity" were able to carry on distinct trains of reasoning, starting, it must be admitted, from false premises, but arriving at distinct conclusions; and these same cases have, after convalescence, been able to clearly recall the events and ideas of the so-called confusional period."

IRELAND, *Torquato Tasso; a psychological study*, Alienist and Neurologist, 1891 XII. 477.

This study is based upon the various lives of the poet Tasso, which we possess in Italian and in English. The writer calls special attention

to the fact that, in the case of this great and unfortunate man, we have fairly trustworthy data concerning his childhood, and are not left in complete ignorance of his ancestry. The vicissitudes of his family were not without their influence upon a child naturally precocious, and it is probable that the seeds of the melancholia, which afflicted him, were early sown. Tasso was a poet at seventeen, and, by the time his great work was ready for publication, the sensitive young man had suffered much from critics and censors. Dr. Ireland points out that the complaints of persecution, made by the poet, remind us of the similar complaints of Jean Jacques Rousseau. We are given a description of Tasso's personal appearance, in which the following facts are to be noted: The orbits of the eyes were unusually large, the head large, the forehead high and sloping towards the top. He was short-sighted and had a slight stammer in his speech. From his own confession, he appears to have been over-indulgent in pleasures and rather fond of good wines. He had a tenacious memory, an unbounded ambition and love of glory, and a keen sense of injustice. He was proud, irritable, and deeply religious. And, added to all these, was the melancholia which often made him feel what he himself describes: "Something, I know not what, is whirling in my mind." These suspicions and aberrations made it impossible for his friends to ignore the affection which was evidently troubling him, so he was sent to a monastery to be taken care of by the monks, from which he escaped before his mental condition had much improved. While laboring under the idea that he was being persecuted, Tasso saw that many of his associates regarded him as insane. To the belief, widely current in Italy at the time, that the poet had gone mad through his love for the Princess Leonora, Dr. Ireland does not attach much credence. Some writers have held that the imprisonment of Tasso, which began in 1579, was the cause, instead of the result, of his mental derangement, but Dr. Ireland concludes that they fail to make out any good case. While in prison, Tasso was still in such a mental condition that he could bewail his misfortunes and overwhelm his friends with petitions. There can be little doubt of the poet's insanity at this time, for Tasso himself describes some "symptoms, the import of which no one acquainted with insanity can fail to read." He complains of the persecution of human and diabolical agencies, and is troubled by the apparition of spirits. His mind at times seems to have been much less unhinged than at others. His letter to Prof. Mercuriale, in the summer of 1583, contains his own account of his malady and the symptoms are unmistakable. Dr. Ireland concludes that Tasso was indeed affected with that form of insanity which is now called parenoia, characterized by a slow evolution of mental derangement, as shown by delusions of suspicion and persecution, hallucinations, and perversions of judgment. There appears to have been no hereditary neurosis in his family, but it is likely that the anxiety which Portia suffered before his birth and the griefs of his childhood helped the development of the mental derangement. After his release from prison the mind of Tasso seems still to have retained much of the great power that characterized it when unimpaired. The rest of the poet's life was filled with unhappiness and misfortune, and for two years, at least, after leaving his prison he was still subject to his strange delusions, and his actions at this time remind one of Swedenborg. He died, after a foreboding of his coming end, and after making the strange request that all the copies of the "Jerusalem Delivered," of which about twenty editions had been printed, should be gathered together and burned.

A. F. CHAMBERLAIN.

TURNER, *Asymmetrical conditions met with in the faces of the insane; with some remarks on the dissolution of expression*, Journal of Mental Science, 1892, XXXVIII. 18.

This is a consideration of "certain asymmetrical appearances, chiefly noticed in the face, by which we can actually demonstrate the existence of paralysis in a large proportion of all cases of insanity." Bilaterally associated movements have been chosen for study, because they are, as a rule, of equal strength on each side. Asymmetry of expression is not, of course, confined to the insane, but is to be found among nervous, excitable people, religious enthusiasts, and, though rarely, in normal individuals. It is possible for some people to "voluntarily produce a most marked asymmetrical condition in the contraction of the muscles of expression." In a case noted by Dr. Turner, there was "a strong tendency for the assymetrical condition to become symmetrical," the subject needing to fix his attention entirely upon what he was doing in order to prevent the contraction becoming equally marked on both sides. "Inequality of the pupils is present in one-fourth of the cases of insanity on admission, and in chronic cases it becomes more common, and it is most common in general paralysis." It would appear also that "the right pupil is more frequently the larger, the difference being very slight in the recent cases, more marked in the chronic." From present evidence it is not possible to state definitely the precise locality in each hemisphere the paralysis of which has led to the asymmetry. "In 306 female cases, recent admissions, the tongue, when protruded, was deflected from the middle line in 80 instances, or 24 per cent." It would seem then that paralysis in the muscles that protrude the tongue prevails to about the same extent as it does in the muscles controlling the size of the pupils. The muscles of expression here considered are, in the upper zone of the face, the *occipito-frontalis* and *corrugator supercilii*, and in the lower, the *levator labii superioris* and the zygomatics. In the cases of fresh admissions asymmetrical action in the upper zone of the face bears to asymmetrical action in the lower zone, the proportion 3.7 to 1. Among idiots, and in all congenital cases of weak-mindedness, the lower zone is most frequently affected. In the cases of insane females the "frequency with which the muscles of expression of the lower parts of the face are called into play under emotional states, which would in the sane result in expression more confined to the muscles of the upper part," is noticeable. In other words, "their expressions are more animal-like, less mental." The paper is accompanied by a plate containing gravures of faces of melancholic and insane women, showing asymmetry of various regions of the face, and an explanatory chart.

A. F. CHAMBERLAIN.

KIERNAN, *Is genius a neurosis?* Alienist and Neurologist, 1892, XIII. (I) 118.

This is a review from the time of Aristotle to that of Lombroso, of the doctrine put forward by the Greek philosopher in these terms, "No excellent soul is exempt from a mixture of madness." The usual examples, chiefly from Lombroso, of the peculiarities and idiosyncrasies of genius are given. Shortness of stature, rachitis, excessive pallor, infirmities of the body, cerebral and cranial lesions, asymmetries, and abnormalities of the skull, stammering, left-handedness, celibacy, precocity, misoneism, errabund habits, etc. Dr. Kiernan concludes, "The alleged intellectual association of insanity and genius would seem, therefore, to be justified," but he is far from identifying the two. "Genius is not a product of morbid mind. In the exceptional instances where the two co-exist, the genius is evidence of a healthy, conservative

element struggling with the incubus of disease." He does not differ then to a great extent from Dr. Paoli, whose apt phrase, "genius with a neurosis" is better suited to the facts of the case.

RIGGS, *Extreme loss of muscle sense in a phthisical patient*, Journal of Nerv. and Ment. Dis. 1891 XVI. 437.

Clinical case, 10 year old girl, phthisical, sensation everywhere perfect, reflexes normal, muscular co-ordination of upper extremities normal, but co-ordination of the lower extremities was noticeably wanting, especially in the muscles of the feet. Treatment by electricity and tonics, recovery from the astasia, but death from phthisis.

Another patient with the usual symptoms accompanying phthisis had lost the power to use the right forearm and hand; death from phthisis. The connection between phthisis and multiple neuritis seems well established.

A. H. PEIRCE.

Harvard.

NOBLE, *Report of a case of anæsthesia of the right side, etc.* Jour. Nerv. Ment. Dis. 1891 XVI. 238.

Patient, twenty-nine years old, had suffered from rheumatism, which had left him with a somewhat involved heart trouble. Upon an occasion of unusually violent exertion he was attacked with anæsthesia of the right side. This was soon succeeded by hyperæsthesia of the same side with paresis of the left side. The symptoms manifested were probably due to reflex action from preputial irritation.

A. H. PIERCE.

Harvard.

G. S. WEIR MITCHEL, *Doctor and patient*, p. 177.

The doctor should not dig up his patient's symptoms. Before and after illness is the time to cultivate those inner morals which pain and weakness usually kill. Women are prone to confess too much to the physician, and illness breeds a passion for confessions and even for distortion. Women warp morally if long nervously ill. Physicians of all centuries have probably agreed most on rest, diet and exercise. The best do what is right, but often give foolish reasons for it. All always look beyond drugs. Instruments to measure temperature, pulse, etc., have improved the doctor's hand, eye and judgment. The doctor must not read the riot act to feelings nor poultice them too much, must not be too sympathetic and must be the grave of all secrets. Few things are so delightful as convalescence. Numberless tissues and molecules are being restored, more rapidly than in the growth of childhood. The physician who has not been ill is imperfectly educated. Pain is usually the prayer of the nerves for healthy blood. People learn it in very different ways. Sickly children are usually spoiled by indulgence. The worse the weather the better the exercise out of doors. Girls should be trained just like boys till adolescence; some even learn to box. Camp life for women with swimming, shooting, climbing, fishing, etc., are recommended.

The sudden insanity of Guy de Maupassant may well raise the question of the legitimateness of the aims of a school of art that seeks to reproduce sensation as its highest aim. If a man of great sensitiveness cultivate it with drugs and excesses, or would go about without his skin, or look straight into the sun, he is not a Promethean martyr to art, but simply lacks physiological common sense. The persistent dwelling, too, on things not of highest significence must also tend to upset the mind. Heine, Baudeclaire, Byron, Musset, Jules de Goncourt and the

long list of gifted men who would not accept the ordinary limitation of mind in body have something clearly wrong about them. Theirs is a wrong theory of art and of life.

Du Maurier's Peter Ibbetson is an autobiography of a man who during adolescence grew morbidly sensitive and bashful, though a man of handsome and imposing presence. He meets a beautiful dutchess, who later turns out to be an old playmate, dreams a striking dream about her, and finds she had dreamed at the same time the same dream of him. Hearing that the man he thought his uncle is his father he kills him and is imprisoned for life. For 25 years his dreams are filled with life with this beautiful lady. They were married, visited many scenes and lands. Everything, their dress, home, conversation were as natural as if real. At length he dreams she died (just as she did in reality do) and has an attack of suicidal mania, and is transferred to the mad-house. An interview with her spirit greets him and he dies as he finished his story.

Dr. S. Weir Mitchell, in his novel, Far in the Forest, describes several types of slight nervous ailment and a curious partnership between a blind and a deaf man, the former being a Swedenborgian dominated by auditory illusions. Paul Preston has the restless vivacity of slightly constructed characters, and like men of his type resembled certain immature feminine natures, and had a like attractiveness. He was easily pleased, and would go any length to escape pain, and a slight but painful malady bred a habit of resorting to opium, a habit most easily made and hardest to break. His moral energy gradually was lost, his property vanished, and even life in the backwoods could not save him. Shortly before death he characteristically lost his love of the drug.

TASTE AND SMELL.

Professor E. H. S. Bailey, Ph. D., University of Kansas.

Les Odeurs, *Demonstrations pratiqes avec l'olfactometre et le pèse-vapeur*, par M. Charles Henry, Paris, 1892.

In this valuable contribution to our knowledge of the odors, the properties of gases and vapors are discussed, more especially those points that refer to the tension of vapor and its expansion by heat. There are some inorganic bodies that possess an odor, but most of the perfumes are of organic origin. Various attempts have been made by Berthelot and others to classify odors, but the task is a difficult one. His classification is based upon the chemical composition of the organic bodies. The question naturally arises, is there any relation between chemical composition and odor? From some studies that have been made there seems to be a relation between the odor and the atomic weight. As the odor is in a great measure independent of the chemical composition, it must depend upon the molecular arrangement of the atoms and this seems to involve the discussion of isomeric bodies, and of the constitution of the more complex hydrocarbons.

There are six methods of extracting perfumes; expression, distillation, maceration, enfleurage, a pneumatic process, and finally, a process by solution in volatile liquids. From the various products thus obtained, the extracts, bouquets, pomades, etc., of commerce are obtained. Eugène Rimmel's table of classification of natural odors is of interest, and is the basis of specific description in regard to the sources of numerous perfumes.

It is supposed that perfumes are propagated by the emission of solid, liquid or gaseous particle, hence the laws of diffusion and of evaporation must be studied. But little is known of the diffusion

of solids, but the diffusion of liquids has been thoroughly studied. It is evident that the volatility of a liquid may be expressed by the weight of the liquid that evaporates per second from a square millimeter of surface, at a given temperature. This weight is proportional to the excess of the maximum tension of the vapor at that temperature over the tension that the vapor possesses in the air, and furthermore this weight varies in inverse ratio to the pressure of the air, there being a special factor for each liquid. On this account evaporation is of value in the determination of the purity of a liquid. The *pése-vapeur* described by the author is really a small hydrometer, floating in alcohol. At the upper end of this instrument is a cup into which the volatile liquid is poured. This cup slides up and down in front of a graduated scale. As the liquid evaporates, of course the instrument floats considerably higher after some time. Ether is taken as a standard of comparison. The laws that govern evaporation, when the surface is not exposed freely to evaporation, as when it is covered with some non-absorbing membrane, are of special interest and have been the subject of a series of experiments by the author and M. Gustave Robin. The instrument called an *olfactometer* gives what may be called the measure of the intensity of a perfume, as it is designed to determine the weight of the vapor that must be evaporated and must find its way into the nasal passages, before the operator can perceive the odor. Indeed, it is proposed to use it to estimate the comparative value of different perfumes.

The olfactometer consists of a glass cylinder supporting, by a cork in its upper end, two tubes sliding the one within the other, the outer tube being of paper and the inner of glass graduated in millimeters, and projecting above the apparatus, where it terminates in a forked tube carrying a glass stop-cock. The forked tube is introduced into the nostrils during the experiment, and a small quantity of any odorous substance is placed in the outer cylinder by a pipette, through the opening that is provided for that purpose. The operator then notes the time, inspires the air regularly, and slowly raises the tube. He notes the time when he first perceives the odor, which has penetrated through the paper, and reads the mark on the graduated tube.

By filling the tube with carbonic acid gas it is possible to determine the proportion of the volume of gas absorbed at each inspiration to the total volume of the tube. By means of a pneumograph it is possible to obtain a record of the inspirations and thus what may be called the coefficient of inspiration. The record is made upon the blackened surface of a paper attached to a cylinder, which slowly revolves. An interesting series of cuts is given to illustrate the effect of inspiration of different odors.

The gustatory organs of Belidens Ariel, Frederick Tuckerman, M. D.—Journal of Anatomy and Physiology, Vol. XXVI. p. 85.

The author describes in detail the circumvallate papilla, with the taste-bulbs, also the gustatory ridges and the fungiform papillæ. In regard to the gustatory ridges of this animal it may be noticed that there are structural characters common to both the circumvallate type of taste-area and the bulb-bearing ridges of *Ornithorynchus*. The ridges of *Belidens* furnish an intermediate stage in the process of development of the former from the latter; the more recent from the more primitive type of the taste-area. Hence an important link in the history of this development is supplied, and now that two types are found together, it is possible that further investigation will reveal the foliate type in its simplest form co-exerting with them.

A note upon the disturbance of the sense of taste after the amputation of the tongue, Frederick Peterson, M. D., Medical Record Vol. XXXVIII. p. 230.

There has always been great difficulty in tracing the course of the nerves of taste, and the distribution of the final taste filaments. It has been pretty well demonstrated that there are special areas of the tongue where certain tastes may be perceived and others that are susceptible to different tastes. The author has had an exceptional opportunity for making some experiments on the phenomena of taste, upon two patients whose tongues had been removed.

The first patient could, when liquids were given him to swallow, taste bitter, sweet and salt substances, but not acid substances. Special tests, made on definite localities, resulted in finding that bitterness was feebly perceived by the soft palate, strongly by the posterior wall of the pharynx, while sweetness was not perceived at all, except on the posterior wall of the pharynx. Saltness was not perceived either by the palate or the pharynx, but probably by the surface of the epiglottis. The galvanic current, when applied to the parts, produced no sensation of taste.

Another noticeable fact was that the sense of smell was entirely abolished after the removal of the tongue, so that the patient could distinguish no odor whatever even in such strong smelling substances as tar, iodiform or oil of wintergreen. In the case of the second patient, whose tongue had been removed, besides the inability to distinguish acids, sweets could not be at all recognized.

The education of the sense of smell, Schneider.—Medical News, Vol. XXXVIII. 452.

Comparatively little has been done in making use of the sense of smell in diagnosis, for the reason that so far no one has been able to classify or describe the different odors. The author looks forward to the day when it will be possible to accumulate and transmit experience in the matter of smell as we now do in reference to the other senses.

Sur les minimums perceptibles de quelques odeurs, M. Jacques Passy.—Comptes rendus, Vol. CXIV. 306.

The experiments made are for the purpose of finding out the smallest amount of any specified odor that is perceptible in a liter of air. A set of standard solutions is prepared, each containing one hundredth as much of the substance as the previous one in the series. Then one drop of the last dilution is introduced into the liter flask, the bottom of which has been previously warmed, to render evaporation complete. After waiting long enough to allow the odor to diffuse itself through the flask, the experimenter smells of its contents and if he cannot distinguish the odor, he repeats the experiment with the next stronger solution, till it is just possible to distinguish the odor. When greater accuracy is demanded, several intermediate solutions may be made up between the last two solutions tested. This method has been proven to possess several advantages, not the least of which is the fact that the observer can make the tests under normal conditions. The alcohol employed should be absolutely pure or an error may be introduced.

The following results show the minimum quantity that is perceptible in a liter of air; the results being expressed in thousandths of a milligram:—

Subjects.	Ether.	Orange.	Romarin.	Wintergreen.	Menth.	Vanillin.
No. 1 Æ	27 0.5	0.05	0.05	0.005	0.0005	0.0006
No. 2 "	22 1.	0.05	0.55	0.005	0.01	0 0005
No. 3 "	32 1.	0.05	0.05	0.005	0.001	0.00007
No. "	42 1.	1 to 3	0.05	0.05	0.005	0.001
No. "	40 4.	1 to 3	0.8	0.4	0.01	0.005

The different stages of the experiment may be defined as 1, smelling nothing; 2d, smelling something that it is not possible to define, and 3, ability to name the odor correctly. The figures found agree quite closely with those determined by Valentin many years ago.

On the nerve supply of the sense of taste, John Ferguson, M. A., M. D.—Medical News, 1890 Vol. LVII. 395.

It is quite generally admitted that the lingual branch of the fifth nerve and the gustatory branches of the glossopharyngeal carry the nerve fibres of taste to the tongue and palate. Are these the real supply to the parts of taste, or do they simply carry nerve fibres of taste to the tongue and palate? The author has had an excellent opportunity to observe a case in which there was a total loss of taste on the left side of the tongue, even to the tip. An autopsy was made which proved that the nerve supply of taste for the top and anterior part of the sides of the tongue comes from the fifth nerve and enter the superior maxillary division of the same nerve. The course then must be from the superior maxillary nerve into the spheno-palatine ganglion, thence by the vidian through the vidian canal to the gangliform enlargement of the facial, along this to the chorda tympani, through the chorda tympani into the lingual, a branch of the inferior maxillary of the fifth.

The conclusion is also reached that the vidian is not a motor root passing from the facial to the spheno-palatine ganglion, but a sensory nerve of the special sense of taste from the spheno-palatine ganglion of the second division of the fifth nerve to the seventh or facial; also nerve fibres of taste for the back of the tongue, fauces and soft palate cannot be carried by the chorda tympani. Proof is also adduced that the root of the glossopharyngeal nerve does not contain any fibres of the special sense of taste.

The route then for the sense of taste, so far as the glossopharyngeal is concerned, would be from the root of the fifth through its third division to the otic ganglion, from this by the small petrosal to the ganglion of the seventh, thence to the tympanic plexus, again by the tympanic branch to the petrous ganglion of the glossopharyngeal and by this latter to the back of the tongue, fauces and palate.

In a criticism on the above article in the Medical News, Vol. LVII. p. 464, by Dr. Chas. L. Dana, he asserts that while the researches noticed seem to prove that loss of taste is due to a disease of the vidian nerve, they do not prove so conclusively that the glossopharyngeal nerve has no gustatory functions.

Sur la physiologie comparée de l'olefaction, M. Raphael Dubois.—Comptes Rendus, Vol. CXI. 66.

The olfactory organs of mollusks have been studied by numerous experimenters, but so far little has been learned of the mechanism of the organs or of their mode of acting. It is a fact that many odors excite the organ of smell of Helix Pomatia, a mollusk well adapted to experiments on the special senses. The large tentacles are more sensitive than other parts, while that of the small tantacles, though considerable, is notably less than that of the large ones. As far as the rest of the external covering of the mollusk is concerned, it is excited by only a few odoriferous substances. There is greater sensibility at the extremity of the large tentacles, though it is apparent throughout the

whole length. The experiments on snails were made by separating the tentacles and closing the end with a clamp, then these were put into a vessel containing moist air and it was noticed that after a time the movements of the tentacles ceased, but if some strong vapor, like that of benzine, was introduced into the vessel the movements would begin again with great activity. From these experiments and others in the same line, the author concludes that it is the sensibility of the segments that is first excited, then this sensation is transferred by contraction, which in its turn agitates mechanically the nerve terminals and is conveyed to the sensorial nerve centers. The first excitation is mechanical, just like that which produces the sensation of touch.

Further observations on the development of taste organs in man, Dr. Frederick Tuckerman.—Journal of Anatomy and Physiology, Vol. XXIV. p. 130.

In the tongue of the human embryo of the tenth week, the organs were so slightly developed as to be hardly worthy of notice, but in the examination of the tongue of the fœtus of the fourteenth week it was noticed that the upper surface was more or less marked by papillary elevations of the mucus membrane. The different layers of the epithelium were also studied. The striated muscle fibres were clearly to be seen, but the striæ were exceedingly faint. Some papillæ of the circumvallate type, in the early stages of development, were present, and the future position of the trachea was clearly indicated. Lateral gustatory organs could be perceived at the sides of the back of the tongue. But little could be learned of the structure of the bulbs in the circumvallate papillae.

Ueber das Vorhandsein von Geschmackiempfindung im Kehlkopf, Dr. P. Michelson.—Archiv fur pathologische Anatomie und Physiologie und fur klinische Medicin. Vol. CXXIII. 389.

The author has studied the special functions of the taste cells of the inner portion of the trachea. He experimented on 25 persons, by putting into the throat upon the end of a bougie concentrated solutions of quinine and of saccharine. Seventeen persons were able to distinguish the bitter taste of the former, three thought it bitterish and the rest were in doubt as to the taste. With the saccharine solution all but three of the twenty-five could recognize the sweet taste. There was one special case in which the bitter could not be detected at all, while the sweet could be readily recognized. Some, in reply to a question as to the locality where the sensation was recognized, said it was where the solution was applied, others that it was in that region, while some simply located it deep in the throat. The electric current was also applied to the same localities, and it was noticed that the application of one pole produced the sensation of an acid taste and the other of an alkaline taste.

Sur la norme de l'acuite olfactive (*olfactie*), Zwaardemaker.—Archives Neerlandaises, XXV. 131.

From the average acuteness of the sense of smell accurately measured in 21 persons with the author's olfactometer, a norm is reached on which is based a system of measuring and recording the acuteness of smell, modeled after that in use by oculists for visual acuteness. The *olfactie* or average *minimum perceptible* of smell is the unit taken for these measurements. The average for a table of proper proportions of these substances and pictures of the olfactometer may be found in the original. When the mixed odors are delivered to the same nostril it might be supposed that they neutralized each other by some chemical

or physical change, but this is out of the question when each nostril receives a single odor, when the phenomenon is no less to be observed. By the adoption of porous earthenware cylinders in his olfactometer, Zwaardemaker is enabled to make tests with any odorous solution of definite dilution and most important of all to make them with chemical substances of known formulæ. It is to be hoped that the author will continue his studies in this interesting and, until his investigations and those of Aronsohn, rather unsuccessfully worked field. The 21 persons (furnishing 34 normal nostrils) is 1.5 degrees on the olfactometer (i. e., they could just perceive the odor furnished by the inside surface of a vulcanized rubber tube of 8 mms. bore when 1.5 cm. were exposed). The figure occurring most frequently, however, was 0.7 degrees; and Zwaardemaker's own *minimum* is 1.0. The figures for the cases examined, as also those for some visual tests made by the author, appear in the original.

Compensation von Gerüchen mittelst des Doppelriechmessers, Zwaardemaker.—Fortschritte der Medicin, 1889 Vol. VII. 721.

By use of his simple olfactometer the author has been enabled to verify, amplify and give numerical exactness to the empirical observation of perfumers, physicians and others that certain odors do not blend but neutralize each other. In this way the odor of vulcanized rubber may neutralize the odor of cedar wood, gum benzoin, paraffine wax, balsam tolu, and in turn be neutralized by any one of them if the odors are rightly graded in intensity; if the right proportion is not observed either one or the other prevails.

SPACE, TIME.

LIPPS, *Die Raumauschanung und die Augenbenegung*, Zeitschrift für Psych. u. Phys. der Sinnesorgane, 1892 III.

This is mainly a criticism of "eye-movement" theory, from the standpoint taken in other writings of the author. Lipps insists on the distinction between what is really seen and what is inferred, be the "seeing" normal or abnormal. Such optical illusions as the apparent inequality of distances and of line lengths are simply a result of our comparison — a matter of judgment. Eye-movements help us to form our space-consciousness by giving clearness and certainty to perception. Furthermore, if we regard the field of vision as a section of the larger space-world, which we survey by moving our eyes or turning our head, we may say that the position (and change of position) of the visual field is measured by such movements, without admitting that the relative position of points in that field is affected.

We do not *see* distance; we *judge* one object to be more or less remote than another. In this, as in other sense-perceptions, we *believe* that we perceive something, which in reality we do not perceive. The *form* of our visual field is likewise the outcome of judgment. The inference, not of our original, but of our developed space-consciousness, is based on eye-movements, and especially upon convergence-sensations. The original field of vision is no more a hollow sphere than it is a plane; in fact, for monocular vision there is no such thing as a spherical field. The "Blickpunkt," which is supposed to sweep round in immense circles, is an abstraction. The points successively fixated range themselves in circles because of the dome-shape of the visual field — a shape given it by experience, insomuch as we perceive no difference in those sensations of convergence, which have come to be distance signs, and therefore regard the objects to which they correspond as equally distant from us.

The consciousness that objects are at *different* distances from us tends to correct our perception of their size. Hence, a conflict between per-

ception and the after-estimate. If the claim of the former could not be repressed, our field of vision would really be spherical; as it is, the *fiction* of such a form is harmless, provided it be not mistaken for reality. The probability of completely setting aside the evidence of perception decreases when the contrast between it and the correction urged by experience is too broad, or when the motives for such correction are not forcible enough. In the latter case, the convergence-sensations are not definite, or are not closely connected with the consciousness of size and distance. The influence of these conditions suffices to explain certain optical illusions without bringing in the eye-movement theory. To our indirect vision, a slanting line seems vertical, because our indefinite consciousness of its position does not force us to correct the perception, i. e., to lengthen in thought the distance of its farther end. The apparent curvature of straight lines is easily understood when we remember that straightness is not given in perception, but is a subjective product, and that the distance-relations out of which it grows are liable to vary with the changing effect of convergence-sensations. When these sensations correspond to the main point of regard, the curvature is less marked, because they indicate with special clearness the position and distance of the points in the line. When they have been trained by experience, as in the case of short distances, they yield an immediate impression of true distance and real magnitude; but when such experience is lacking, as it must be for greater distances and very acute angles, their worth, as distance signs, is merely analogical.

If we fixate the middle-point of a straight line, without regard to any point outside, the contradiction between perception and reality is less striking; the naturally favored straightness asserts itself; there is scarce an appearance of curvature. The illusion is more striking when we view the line with reference to a point outside, because the contradiction is greater. Observation of lines that seem to bear towards the eye, concavely or convexly, shows that the chief point of regard and our "spatial middle-point" *may* coincide, but not that they necessarily do so. Our consciousness of curvature is therefore variable, and is conditioned, not by the laws of eye-movement, but by our own mode of apprehension. The same holds good of our space-estimate and its results. The ground seems to rise towards the horizon, not because we raise our eyes, but because we underrate the distance of remotest points. The right eye undermeasures a line on the left, and vice-versa, because owing to the acuteness of the angle, we undervalue the distance differences between the line and the remoter eye. In binocular vision, the nearer eye guides our estimate according to the principle of "habitual average valuation," and the judgment thus formed affects monocular vision. The same principle accounts for errors in measuring vertical distances, and for the over-estimate of horizontal distances on the left as compared with those on the right.

E. PACE.

DE MEMME, *L'ipotesi degli spazi a* n *dimensioni in rapporto con la psicologia e la gnoseologia*, Riv. di filos. scient. 1891 (2) X. 688.

On the principle that geometry of *n* dimensions is merely algebra written in metaphor, De Memme criticises the hypothesis of Helmholtz and its application, by De Saussure, to physical and chemical problems.

FALK, *Versuche über die Raumschätzung mit Hülfe von Armkenegungen.* Inaug. Diss., Dorpat, 1890, p. 58.

Falk studied the absolute and relative error in judging space distances by a movement of the forearm. The forearm was supported from elbow to finger-tip in a convenient carriage moving along a slide; this carriage

could be stopped at either end at desired points so that the distance moved over could be conveniently and accurately read off. By aid of certain appliances the apparatus was serviceable for the methods of right and wrong cases and of the average error. There was also used a combination of the method of right and wrong cases, and of the just observable difference, which has nothing in its favor and its complexity against it. At other points the author is too much dominated by methods hardly applicable to the variable character of his results. The movements varied from 1 to 20 cm., and were made with the shoulder joint as a pivot, moving through an angle of from about 2° to 40°. Falk studied the effect of the rate of movement and the weighting of the carriage upon the constant and the variable errors. The attempt to reproduce distances of 1 cm. resulted in an exaggeration of 81% of 25 cm., 33% of 5 cm., 12.4% of 10 cm., an underestimation of 0.45%; and of 20 cm., 0.82%. The movements forward or away from the body are somewhat more accurate than movements backward or toward the body. Weighting the carriage with from 100 to 600 grms. does not appreciably affect the constant error. Passing to the variable error the measure of sensibility is not constant; when expressed by $\frac{1}{12}$ for 1 cm., it is $\frac{1}{19}$ for 2.5 cm., $\frac{1}{36}$ for 5 cm., $\frac{1}{47}$ for 10 cm., and $\frac{1}{57}$ for 20 cm.; the smaller movements showing the least sensibility. Movements forward show rather finer sensibility than movements backward. The rate of movement has only a slight effect upon the percentage of right judgments, that of 6 per minute having a slight advantage. The effect of weighting the carriage is also insignificant. Weber's law does not hold within the distances measured. The curve of movement as recorded on a drum and shown to gain slowly, reach a period of constancy and maximum rate, and again fall off.

BONNIER, *Physiologie du nerf de l'espace*, Comptes rend. 1891 CXIII. 566.

An acoustic disturbance coming from a given direction arrives at the ear under a certain angle of incidence, is reflected by the concha and the walls of the meatus externus and reaches the tympanum under a new angle of incidence which for a given end depends on the original angle. The concave and conical tympanum is driven back in the axis of its cone, if the noise arrives in that direction, and oscillates sidewise if the sound arrives in an oblique direction, drawing after it the point of the hammer. The system of the anvil and hammer forms a bent lever suspended on an axis that, thanks to the articulation of the joint, itself bends in the form of an elbow, and can turn in any direction. On both sides of this articulation there are three pivots, two for the hammer and one for the anvil. The external process of the hammer serves as pivot for the lateral downward oscillations of the hammer and as axis for the backward oscillations. The short process serves, above all, as axis for the movements from without inward, and as pivot for oscillations in any direction. The superior posterior process of the anvil serves as its pivot for all movements of bending in the central articulation and as axis for the direct oscillatory movements of the entire system. The articulation permits motion in every direction while yet retaining the total oscillation from without inward. According to the lateral oscillation of the point of the hammer, the system bends so that the surfaces of articulation quit each other at some points to meet at others, in such a manner that the angle formed by the two free arms varies in planes equally variable. The point of the anvil transmits by a double articulation its oscillations to the head of the stirrup, which in its turn oscillates around its tendinous insertion, pushing the base of the fenestra ovalis according to its various inclinations, which are the reverse of those of the point of the anvil, but always without disturbing the backward compression. According to its obliquity the plate of the stirrup

in forcing itself into the opening tends to depress such or such a pole of the adjacent utricular convexity. According to this compression, and according to the pole depressed, a circulation of the endolymph is caused in the utricule. This provokes corresponding currents in the semi-circular canals. These compensating currents vary in direction and intensity, depending on the direction of the displacement in the utricule, the pole depressed and the direction of incidence of the disturbance. The cristæ of the ampullæ perceive on opposite sides the direction and the intensity of these currents, which reconstitute in a manner the geometric description of the utricular disturbance. The nerve of the flat macula perceives the intensity of the disturbance which arrives diametrically from the convex wall, while the three nerves of the ampullæ analyze its direction. The so-called nerve of space is then only the nerve of disturbed space and of sonorous space. It defines the position of the points perceptible by the ear by means of the disturbance of the interposed medium. It localizes objectively the origin of auricular perceptions by the direction of the incidence of the disturbances.

E. W. SCRIPTURE.

SCHUMANN, *Ueber die Unterschiedsempfindlichkeit für kleine Zeitgrössen*, Zeitschrift für Psychologie und Physiologie der Sinnesorgane, 1891 II. 294.

The experiments were performed by the methods of right and wrong cases and the average errors. The clicks which gave the time interval were produced by the momentary passage of a current in a telephone. The closing of the current was done by contact with platinum points on a regularly revolving wheel. Time intervals from 0.15 sec. to 2 sec. were experimented upon by the method of right and wrong cases. The discriminative sensibility was found to be greatest for 0.3 to 0.4 of a second, a result in agreement with that of Mach. By the method of average errors experiments were tried upon intervals from 0.5 to 5 sec. This method, however, cannot be employed for solving this problem, because the average error is very great with intervals between 0.3 and 0.4 seconds, whereas, the discriminative sensibility was found to be finest at this point by the method of right and wrong cases. Moreover, comparison and reproduction of small time intervals are different operations.

BERGSTRÖM.

EPSTEIN, *Die logischen Principien der Zeitmessung*, Leipzig, 1887.

The author reviews the opinions of Newton, Locke and Leibnitz; and at the end of his article some of the recent mathematical definitions of equal times. He approaches the problem from the side of the theory of knowledge. Time is an auxiliary variable introduced by us into the phenomenal world to give order to its events or processes. Equal times are those in which identical events take place. But we have no criterion of identical events and must content ourselves with considering those events identical for which the contrary hypothesis would be less reasonable.

BERGSTRÖM.

METTLER, *Aural vertigo (Menière's Disease)*. Journ. Nerv. Ment. Dis. 1891 XVI. 19.

There is no sufficient reason for supposing that the semi-circular canals or any other definite organs are the seat of the sense of equilibrium. The feeling of equilibrium is due to the harmonious relations of the sensory centers to each other and to the motor centers connected with them. Any serious injury to the centers may bring about the mental confusion and motor ataxy which we call vertigo.

BERGSTRÖM.

G. MOURET, *Le Problème d'Achille.* Revue Phil. Jan. 1892.

According to Mouret, Zeno is guilty of a "*petitio principii*," because he seeks an unknown quantity which his very method excludes. The error permitted in dealing with convergent series should have been rectified by passing on to the limit, which in this case is zero, and consequently implies a position common to the tortoise and his pursuer. This correction once made, it is clearly possible for Achilles to overtake the tortoise; whether he actually does so is a question that depends not on the relative, but on the absolute, velocities. If these do not approach zero, Achilles will succeed within a determined time; if they do approach zero, he will never succeed, but his failure implies nothing at variance with the concept of motion.

EPSTEIN, *Die logischen Principien der Zeitmessung.* Inaug. Diss., Leipzig, 1887.

On the side of empiricism, Newton distinguished between absolute and relative time. The absolute or mathematical time has a uniform flow; the relative time, measured by cosmic or artificial motion, is subject to irregularities because the motions vary. Absolute time, like absolute space, matter, and motion, are not abstractions needing justification, but the veræ causæ of the corresponding relative facts. None of our measurements correspond to the actual or absolute qualities. The error takes different directions for time and space. We cannot perceive empty space, only space as occupied by bodies. If these were at rest space could be mapped out with accuracy; but since they are probably in constant motion, the point from which we measure is liable to change. Motion is the only measure of duration; if it were uniform, it would measure absolute time; but no uniform motion is known, so in the case of time our unit of measure is liable to variations.

On the psychological side Locke came to a similar conclusion. A notion of time is gained from the succession of ideas. From periodic sensations we derive a unit of measure, which we extend to all phenomena in which such a unit is absent. Not simply motion, but all periodic phenomena furnish a unit of time. The difficulty with time measurements is that we know no uniform motion or regular periodic process. The year, the day, the swings of the pendulum—all vary. A second difficulty peculiar to time is that one stretch of time cannot be superposed upon another, while this method of superposition is the foundation of space measurement. The author shows that this class of objections, though of special force in regard to time measurement, applies to the measurement of any two different parts of the same continuum, whether time, space or motion.

To apply geometry practically we pass from the absolute to the relative space by two axioms, which may be united as follows: A body under the same conditions, at different places, or at different times, occupies equal spaces Practical time measurement is founded on a similar axiom. An event under the same conditions at different times or at different places has the same duration. The empirical and naïve psychological theorists considered time a substantial thing whose nature was to be investigated, and found discrepancies between the absolute and relative or practical time measurements. The author treats the problem from the point of view of the theory of knowledge. Time is not to be considered a substance, but as a category introduced by us into the phenomenal world to give order to events. The phenomenal world is united into a whole by the causal category; any given value of it determines the next. Time is an independent auxiliary variable introduced to fix any given stage or value of this phenomenal world, and to enable us to pass connectedly from one value to another. It is the

means of a systematic view of natural processes. The question what is time, or what are equal times, is a matter of definition. Equal times are those in which identical events take place. Time is concerned only with events, and we know nothing of empty time. There is no criterion of identical events independent of time, and we must be content with assuming that two events are identical when it is more reasonable to assume that than the contrary. The paper closes with a criticism of the recent mathematico-physical definitions of equal times.

TOUCH, PAIN, INTERNAL SENSATIONS.

GOLDSCHEIDER. *Ueber die Summation von Hautreizen*, Dubois-Reymond's Archiv 1891, 164.

Lightly stimulate the skin with the point of a pin, or even a somewhat blunter instrument. A pricking sensation arises, dies away, and is succeeded by a secondary, or after-sensation, which also has the prickly feeling, but lacks the touch-tone which marked the primary one. It seems very much more as if it came from within. If the stimulus be stronger, but at the same time not quite strong enough to make the primary sensation painful, the secondary sensation will be felt as painful. But if the strength of the stimulus be still further increased so as to give the painful tone to the primary sensation, then the secondary is feebler than the primary, and does not flash out as clearly, since the interval between the two is partly filled by the persistence of the primary sensation. Goldscheider found that a single electric shock in no case gave rise to this secondary sensation. In order that it should arise it was necessary to make a number of such stimuli, one after another, on the same spot. This gave a clue to the explanation of the above phenomenon, and also suggested lines of experiment. He and Herr Gud sought to determine under what conditions a series of stimuli is competent to produce the secondary sensation,—how long the series must last, what the interval should be between the shocks, and what the intensity of the current. A series of four gives a clear secondary sensation, but only with certain intervals between the stimuli. With an interval of from 0.03 to 0.06 second, the sensation is most clear. As the interval is made smaller than the former number, or larger than the latter, the secondary sensation becomes less and less clear, finally disappearing altogether. The like is true if the number of stimuli in the series be increased. For each number a certain range of interval is found which gives the secondary sensation the maximum of distinctness. This interval varies, however, inversely with the number of the stimuli. The product of number by interval is found to be (nearly) constant.

In each case a certain intensity is found best fitted to give the secondary sensation. Increasing or diminishing the intensity beyond this point enfeebles the effect. The time elapsing from the end of the series to the rise of the secondary sensation was measured and found to be (on the hand) about $\frac{9}{10}$ second. Increasing the duration of the series by increasing either the number of stimuli or the interval between them has no appreciable effect on this time, until the point is reached when the number of stimuli given is just the number necessary (with each particular interval) to bring out clearly the secondary sensation. If we increase the number beyond this point, the secondary sensation comes out independently of the duration of the series, but with its same time-interval. That is, increasing the duration of the series beyond this point, the interval between the end of the series and the rise of the secondary sensation is correspondingly shortened. When the series is made to last about $\frac{8}{10}$ sec., the secondary sensation flows into the primary, or does not arise at all. Similar results are found with

mechanical stimulation. The reaction time is in effect the same, provided the intensity and duration are made as nearly as possible the same as with the electrical stimulation. Increasing the duration of the stimulation up to a certain point has no appreciable effect on the interval between the *end* of the stimulation and the rise of the secondary sensation. But when a certain point has been reached, then further increase in the duration has just the opposite effect. The interval from the *beginning* of the stimulation to the rise of the secondary sensation remains constant, while the interval measured from the *end* of the stimulation diminishes. The explanation, here, is the same as in the case of electrical stimulation; when the total stimulation has reached the intensity necessary to awaken the secondary sensation, a further increase of the stimulation has no effect on the time. The time required for the rise of the secondary sensation was found to be on the wrist shorter, and on the sole of the foot about half a second longer, than the time given above.

The secondary, or after-sensation, is a "summation-phenomenon." A mechanical stimulation, since it is competent to produce this secondary sensation, should never be regarded as a single stimulus, but always as a series of stimuli. Where this summation takes place it is impossible to say with certainty, but Goldscheider argues that it most probably takes place in the cellular elements scattered throughout the nerve tract. Each stimulus takes two paths; one goes directly to the centre of consciousness. the other is impeded on its way by the cellular elements and only succeeds in making its way on to the centre under certain conditions. The cells "store" the energy, and a series of stimuli is needed to make the stored energy amount to enough to express itself in action. When this does happen, however, these cells send also their message to the centre, but by a different path, which we may appropriately call the "summation-path." This summation-path, via the gray matter of the spinal cord, is the same as has been called the "pain-path." A single stimulus, if of exorbitant intensity, may be sufficient to break its way through this pain-path. When this is the case, we may have both the primary and the secondary sensations coming over the same path; in the original experiment, when we made the stimulus strong enough to give the pain tone to the primary sensation, we still had the secondary or summation-phenomenon, though, in this case, weaker than the primary.

CHAS. M. BAKEWELL.

Harvard.

SERGI, *Su alcuni carratteri del senso tattile, Osservazioni sperimentali.* Rivista di filosofia scientifica, 1891 X. 590.

A series of electric forks of 50, 100, 250, 435, 500 and 1000 v. was employed, a rigid and obtuse point of brass being subsituted for the long fine point used for writing on the drum. The point was applied to some point of the skin to test the limit of rapidity of the blows given by the point that could be perceived as separate. The fleshy part of the fingers could perceive the stimulus as a succession of blows up to 1000 per sec.; many other parts of the hand, the point of the tongue and the red parts of the lips perceived 500, the greater part of the skin perceived 435 per sec. as a succession, which increased in clearness down to 50 v. The question of the intensity of the excitation and the special sensitiveness of the organ stimulated are then taken up. The final results of the experiments can be summarized as follows: 1. The cutaneous surface is not everywhere equally sensitive to tactile stimuli of small intensity; the most sensitive parts are always the palmar extremities of the fingers. 2. Many parts of the skin, although giving a definite sensation of touch, do not give it in the same clear and distinct

way as the digital extremities of the hand. 3. The minimum energy of stimulation, or the least perceptible stimulus, is variable according to the different points of the surface of the skin; in the successive stimulations at equal intervals, as with the tuning-fork, the only sensation which results is not produced by the fusion of the impressions through their persistence, as Bloch would have us believe, but through the insensibility to weak stimuli; that is why the stimulating point is felt as being firm if it is in immediate contact with the skin. 4. In tactile sensations properly so-called, that is, those of the skin, it seems that there is no persistence of the impressions, when the stimuli are limited to and produced by an obtuse point. This does not say that there is an accumulation of small impressions to produce a single result, as usually happens for some other senses and for electrical cutaneous stimulation (Richet). These phenomena on the contrary occur when there is a transformation of tactile impressions into sensations of pressure, i. e., when we cause an obtuse point to penetrate into the surface of the skin by pressure, and when there is a powerful stimulus with a large surface of special character. 5. It appears also that there is not any primary action, or period of latency, in the tactile sensations by which the sensation is developed in a shorter or a longer time, as happens in the case of the retina; it seems that excitation of a purely mechanical nature produces its effects immediately, and if it does not produce them at the first moment, then there is no perceptible effect; one can explain in the same manner also the lack of persistence, in addition to action of a weak stimulus, by the absence of the time of latency in the terminal organs of touch. If there be a primary action it must be of a duration absolutely incalculable, since a series of sensations with an interval of 1-1000 sec. can be perceived. 6. On the mucous membrane of the glans penis there is no sensation of a tactile character such as is found in the skin.

E. W. SCRIPTURE.

SERGI, *Ueber einige Eigenthümlichkeiten des Tastsinns*, Zt. f. Psychologie u. Physiol. Sinn. 1892, III. 175.

An unacknowledged translation of preceding. E. W. S.

BUYS, *Recherches experimentale sur la sensibilité de l'ovaire*, Archiv. ital. de Biol. 1891 I.

Chaiguot had found that, in very many cases, during the later weeks of pregnancy touch upon the abdomen resulted in temporary, but clearly localized, pain of a peculiar kind. As exceptions were not wanting, it was left to decide whether the painful cases were due to disease of the ovary or simply to the greater sensitiveness of the individual. By means of an incision from behind, Buys contrived to apply an induction current to the ovaries of six dogs, and also to use direct mechanical stimulation. Strong irritation of the ovary produces intense pain, dilation of the pupil, increase of blood pressure through vascular constriction and slowing of the pulse through increase of the tonus of the pneumogastric. We are thus led to believe that numerous morbid alterations of these organs in certain circumstances influence circulation by affecting the vaso-motor centers. Thus may be explained, through ovarian irritation, disorders of local circulation so common during the menstrual period of life, or at the close of it.

Harvard. J. F. ANGELL.

BITOT ET SABRAZÈS, *L'analgésie et l'atrophie des testicules dans l'ataxie locomotrice progressive*, Rev. de med. 1891.

This is a critical account of a number of cases of locomotor ataxia, from which conclusion is reached that analgesia of the testicles is so

frequent—though not invariable—under these conditions as to point strongly to a causal connection. This is indicated by the number of cases in which the analgesia appears at the inception of motor-inco-ordination.

Harvard. J. R. ANGELL.

H. NOTHNAGEL, *Schmerzhafte Empfindungen bei Herzerkraukungen*, Zeit. f. Klinische Medecin, 1891, ss. 209.

This article is mainly of value to the physician who would use the painful sensations in the cardial regions for the purpose of diagnosing heart affections in cases where the ordinary physical examination fails to reveal the cause of the trouble, and where, at the same time, there seems to be no functional nervous disorder which would account for these sensations by referring them to a "subjective" origin. Dr. Nothnagel concludes that the muscles of the heart are less concerned in the production of these sensations than the general circulatory system, but thinks that, further than this, it is impossible to reach any precise and satisfactory conclusions, owing to the complicated nature of the cardial nervous system. The cutaneous hyperalgesia and neuralgia found in some cases, he refers to as eccentric spreadings from the main seat of the disease such as we often find in diseases of the vital organs.

MISCELLANEOUS.

CORRE, *Crime et Suicide*, Paris, 1891.

After having in another work studied the criminal from the natural-history and medical point of view, the author proposes now to examine the genetic conditions of antisocial impulsiveness. The book consists in a general etiology of crime completed by a parallel etiology of suicide. As crime varies according to time and place, it is not definable from the act, nor from an antithesis to morality, which is everywhere conventional. The variability of sentiments is precisely the cause of the different interpretations given to correlative or derived actions. The opinion of the majority creates morality and laws. Natural crime is a conventional offense to the average opinion of each collectively. The characteristic of criminals resides in non-conformance to the social obligation of the moment. Man possesses just enough free will to regulate the modes of his collective relations under the form of a contract of necessity. There is no more responsibility in the absolute sense of the word than there is absolute liberty. Under the influence of alcoholism and of social perturbations, crime and insanity present a parallel evolution; the same causes which prepare or determine one affect the other. These diverse forms of aberrant or antisocial impulsiveness spring from the same solicitations. As one observes collective crimes under imitation and suggestion by example (crowds), so he also discovers collective insanity or demoniacal epidemics. It is true that degeneracy of all forms furnishes a large proportion of criminals; yet there is a sufficiently frequent manifestation of impulsivity in individuals of feeble mind or feeble moral resistance. The criminal is not a retrogression, but he is incomplete in evolution. The author does not admit a criminal type in the anthropological sense, but there are criminal types in the psychological sense; one, which comprehends the majority of criminals and includes a part of the criminals by occasion, is latent. The passionate type comprises those who are not degraded, but who are suddenly surprised by a criminal solicitation in a moment of exceptional need. There is another type including subjects of an unstable character, in whom reflection, after the first omission, is not sufficient to hinder a second fault; they are without hereditary taint, their criminality is acquired. A third type consists of those

degenerated through vicious habits, led astray by prostitution, debauched by temperament or by satiety of pleasure, the alcoholics, the morphimaniacs, etc.; under the influence of a special habit a criminal habit is born and developed. The fourth and last type is hereditary, sometimes susceptible of being referred to atavism, not of an ethnic type, but rather pathological, degenerative or teratological.

The suppression of a unity, or its reduction by lessening its conditions of resistance is a crime, from the social point of view. But is it a crime when it is the suppression of one's own proper person, or suicide? To prefer to die, rather than live in a slow degradation under the chains of misery, does not merit reproach but pity, and in some cases admiration. The suicides are the discontented; they do not oppose their surroundings, they simply abandon an environment which they believe themselves unable to inhabit. They do not differ much from those who, disgusted with the world, without knowing the world, at the age of virility, shut themselves in cloisters, protesting thus against the form of general society. Crime and suicide are two acts equally prejudicial to collective interests. They represent two modes of antisocial impulsiveness, which cannot be regarded as equivalents, although they arise from similar psychical aberrations and from conditions more or less analogous. Suicide has more alliance with insanity than crime has. Degenerative habits, as alcoholism, morphimania and debauch are the most intensive causes of attacks against others and against one's self. Alcoholism conducts men to suicide or to crime, according to their characters.

Washington, D. C. ARTHUR MACDONALD

LYDSTON AND TALBOT, *Studies of crimnals.*—Alienist and Neurologist, 1891, XIII. 556.

These deal with the "degeneracy of cranial and maxillary development in the criminal class," and are accompanied by illustrations of criminal skulls and histories typical of the physical degeneracy of the criminal. They are peculiarly valuable and interesting for comparison with the studies of Lombroso and other European criminologists. Talbot investigated a very large number of cases of habituals and murderers among the prisoners in the Joliet penitentiary. The histories and descriptions of these are given in detail, besides the results of examinations of the jaws and teeth of 477 criminal subjects (468 males, 9 females). Lydston presents also the results of his studies of the skulls of criminals and social outcasts of various nationalities and races.

The more pronounced criminal types are found amongst the imported criminals. Simplicity of gyres in the brain seems indicative of degeneracy. The most striking features of the criminal skull, as seen in American prisons, are the tendencies to brachycephalism and submicrocephalism and the great frequency of cranial asymmetry. The direction of the degeneracy is modified by racial characteristics. The dolichocephalic type, when degenerated, becomes in general more dolichocephalic. A striking feature of the degenerate skull, as illustrated by many skulls in the habitual criminal class, is its peculiar "twisted" conformation. Alcohol is the touch-stone, the crucial lymph, that brings out the inherent infection of madness, crime, or bestiality. Heredity is the latent power and alcohol the potential energy that drives the arrow to the mark. Left-handedness is rare amongst American and foreign-American criminals. One per cent. only of the 400 criminals in the Joliet penitentiary were found to be left-handed, and two per cent. in the New York city prison.

It is impossible to look over the descriptions of these criminals without noting the continual occurrence of facial and cranial asymmetries. The physical degeneracy and often the bad heredity of the subjects are

plainly shown, and their defective or degenerated mentality seems often as clearly indicated. With respect to the eyes of eighteen criminals, defective vision is recorded in five cases; in five cases one eye is perceptibly larger than the other, and other anomalies of development occur. Two cases of defective hearing are noted, and in no fewer than eight cases asymmetry of the ears prevails, besides other cases of auricular anomalies. The number of cases considered is obviously too small for general conclusions, but the facts cited seem in general harmony with the results of previous investigators.

The question of the asymmetry of the skeleton and of the bodily organs deserves to be studied more closely. From an examination of the jaws of 468 male criminals, Drs. Lydston and Talbot found that but 163 could be called normal, the rest departing from the normal type in one or more respects. Most of the deformities of the jaws and teeth were confined to the upper maxilla.

The craniological studies of Dr. Lydston seem to have covered a wide ground. In the skulls of these criminals and offenders against organized society, asymmetry and anomalies are equally as prominent as in the case of living delinquents. Some very interesting skulls have come into the possession or fallen under the observation of Dr. Lydston. Among these are the skull of the first Chinese suicide in America, a man who shot himself because the white girl to whom he was engaged proved false; the skull of a negro panel worker, the consort of a noted Chicago courtesan of other days; the skull of a notorious member of the *demi-monde* of Chicago, who was half Indian, half white; and the skull of a noted western desperado and train-wrecker. Upon the study of such material the authors have based their conclusions, which may be summed up in their own words: "As far as our observations go, they tend to show that a degenerate type of skull is common among criminals, and that the assertion of Lombroso, that the deviation of type, as far as the index is concerned is toward brachycephalism, is correct."

A. F. CHAMBERLAIN.

C. S. MINOT, *Senescence and Rejuvenation*, Journ. of Physiol. No. 2, 1891.

The fundamental properties of living organisms constitute the most fruitful theme of biology. From the age of Zero, or the moment of impregnation, animals and plants pass through a series of changes till, barring accident, they reach their limits of longevity. Rejuvenation is procreation. The author weighed, daily, hundreds of Guinea pigs. Gestation is shorter the larger the litter. There is a progressive love in the power of growth, beginning almost at birth, and suggests whether, in all animals, the impulse given at impregnation does not gradually die out. This is indicated by the author's very interesting curves. Curves at least is his theory of "physiologically equivalent weights."

DEMENY, *Analyse des mouvements de la parole par la chronophotographie*, Comptes rend. 1891. CXIII. 216.

M. Demeny gives an account of the application of the chronophotographic process, to the analysis of lip-movements in speaking; and of the construction of a zootrope, by means of which he succeeded in so synthesizing those movements that a deaf-mute, standing before the instruments, was able to read the phrase, whose corresponding lip-motions were thereby reproduced. Vowel and diphthong movements were interpreted as well as those of labials. The experiment was not a complete success; but this was due to the fact that part of the phrase pronounced was imperfectly photographed, and the deaf-mute was not quick enough to guess it from the general sense of the passage. Besides, the movements of the tongue were only very vaguely reproduced, and

consequently read with difficulty. The experiment is most successful upon deaf-mutes; since they are more accustomed by their peculiar condition to interpret mouth movements by sight alone than are normal individuals. Mr. Demeny expresses the hope that the zootrope, improved and perfected, may be of great value in their education.

F. TRACY.

Univ. of Toronto.

HENSEN, *Die Harmonie in den Vocalen*, Zt. f. Biol. 1891, XXVIII. 39.

HENSEN, *Nachtrag zu dem Aufsatz: Die Harmonie in den Vocalen*, Zt. f. Biol. 1891, XXVIII. 227.

The problem proposed is: why is this fundamental tone always absent in the case of vowels produced in singing? Hensen imitates the arrangement of the pharynx and the mouth by a reed-pipe in connection with a resonator. The pipe sounds only with a certain pressure of air. If the resonator is brought into connection with the pipe while the air-pressure is still too small to cause the pipe to sound, the tone of the resonator is heard; as soon, however, as the pressure is great enough for the pipe to sound, the resonator tone ceases. The experiment can be tried in another way. The resonator is held to the ear; its own tone ceases as soon as the pipe sounds. Brought into connection with a manometric flame, and made to vibrate by an appropriate tuning-fork, the resonator shows its tone; but as soon as the pipe is sounded the resonator does not respond unless both are arranged for the same tone. These experiments all seem to prove that a sounding column of air, such as that in the buccal cavity, is incapable of bringing out the tone of the cavity in which it is contained in addition to the tone impressed upon it. The latter part of the former article of Hensen's and the whole of the second one are occupied by a discussion with Hermann.

E. W. SCRIPTURE.

JASTROW, *The Natural History of Analogy;* Address before the Section of Anthropology, American Association for the Advancement of Science, at the Washington Meeting, August 1891. Salem, Mass., 1891, pp. 23.

Prof. Jastrow's address serves to show the close relationship which exists between anthropology and psychology and to emphasize the importance of studies in the field of psychical anthropology. "Deeper than the language of words and underlying their use and formation is the habit of comparing object with object, of tracing resemblances and noting contrasts. It would seem that in the savage's use of this process there is lacking the distinction between the resemblances inherent in the objects and those originating in the mode of viewing them; subject and object are still merged in a vaguer realm of thought, where myth and science, poetical fiction and evident fact mingle without let or hindrance." Prof. Jastrow proceeds to illustrate, by examples selected from all over the world, "the *rôle* that analogy plays in primitive circles, the essential influence it exerts over thoughts and customs in the early history of mankind." Witchery and sorcery, cannibalism, magic, astrology, dream-interpretation, name giving, etc., are shown to rest upon a general basis of analogy. Reasoning by analogy is next considered. Its use by children, in dream-interpretation, in astrology, in the doctrines of sympathy and of signatures, folk-medicine and the like is indicated. The general conclusions are: "Analogies which are but fancies to us were to men of past ages reality (Tylor)." The principle that what was once the serious occupation of men becomes in more advanced stages of culture the play of children, or is reduced from seriousness to mere amusement, finds illustrations in

the mental as in the material world. "The formidable and trusted argument by analogy finds its proper field in riddles and puns." "In such exercises of fancy we are employing the same faculties that our ancestors used in arriving at the customs and beliefs that we have been considering. The laws governing the progress of industrial arts, of mechanical inventions and social institutions seem thus to find equally ready application to the evolution of habits and customs in the mental world."

A. F. CHAMBERLAIN.

PILO MARIO, *Il piacere estetico eola fisiologia del bello*, Riv. di filos. scient. 1891 (2) X. 599, 667.

Pilo insists on the popular definition of the beautiful—that which pleases—and shows that other definitions, especially Mantegazza's, can be reduced to this. Genetically speaking, beauty begets beauty, whether the suggestion be of the present or of the past. The impression produced by the simultaneous action of various æsthetic forces is not their simple sum, but their resultant. Beauty, like goodness and truth, being relative, has no absolute standard. Ruled by the laws of *heredity*, the æsthetic sense varies according to pace, sex and age—now strong where the moral and intellectual faculties fail, now weak where these are marked. In the environment, culture, art and public taste are determining influences. Finally, the need of change brings about, by natural selection, new phases of art and of appreciation.

E. PACE.

Washington.

J. JAURÈS, *De la Réalité du Monde Sensible*, Paris, Alcun. 1891. p 370.

The reality of the external world is not a mere dispute of the schools because the mind had asked itself this question before there was a scholastic tradition, and before curiosity had been artificially refined. The book is a thesis for the doctorate at the Sarbonne. The author was known as a political orator, and his work is here marked by an elocution of style which caused P. Janet to compare it with a symphony. It adds little that is new, and its solution is substantially that of Thomas Aquinas.

ARRÉAT, *Psychologie du Peintre*, Paris, 1892 p. 264.

This is a series of etchings in ink such as one would like to read at Barbizon. The author's own words take up the least part of the book, for he allows the painters themselves to do the talking; and if we hear the same voice more than once, the repetition comes of the arrangement. In five parts, A. delineates the physique, the vocation, the mental qualities, the character and the pathology of the painter. The artist has a certain air about him by which he is easily recognized in a crowd; but to say just in what this consists, to single out a typical face, is not easy. Physiologically, there is no uniformity beyond a nervous excitability, which often leads to excess of various kinds. Nor is the painter's genius always inherited; for though, out of a list of three hundred, two thirds are descendants of painters or artificers, there still remains a considerable number whose ancestry had no artistic bent. But whatever its origin, the painter's vocation, with its peculiar æsthetic traits, asserts itself at an early age. It is shown, as a rule, in precocious children, quick to admire and keen to analyze the beauties outspread to the eye. Impressions thus received fasten on the imagination; the visual elements and the motor elements of memory unite; the hand is as true in reproducing as the mind in retaining. The particular elements, however, which are imaged and transferred to canvas depend

upon a previous analysis of the sight-presentation. In the infancy of art as in that of the individual, outline alone is regarded; then come relief and, finally, color.

This growth in the appreciation of visual impressions goes on, it would seem, at the expense of higher powers. With some rare exceptions, painters are intellectually poor. Lacking in general culture, in judgment, in the scientific grasp of the very principles which underlie their art, they more often possess a strong emotional memory and a consequent partiality for the kindred arts of music, architecture and poetry. Not that they are incapable of thinking and writing on matters of their own profession; the numerous extracts from their letters, which A. presents us, are much to their credit. But with all their powers of invention, they seldom appear to advantage in the field of literature. The brush is their pen. The message it bears to the world is tempered of course by the artist's personality. But he, in turn, feels the influence of his environment. Nationality and climate have a visible effect on inspiration. It is not nature alone that furnishes the theme. The pages of a Shakespeare and of a Byron, the facts of history, even political changes, have aroused the painter's genius. Religion, especially, in its various forms, has been a fruitful source of inspiration. From Raphael to Baudry, from Michael Angelo to Millet, it is the religious element that dominates.

Under all these influences, the painter's individuality must not be forgotten. His character, as pictured by A., is not without its shadows. Comparatively few artists have been tainted with the lowest sort of egoism—the greed of gain; nearly all have felt the thirst for fame, and this, though it check more sordid impulse, rarely suppresses the promptings of vanity and jealousy. Such vices, however, do not dry up all sympathy, nor deaden the sense of duty to parents and family. The painter's first love is his art; to other loves he is neither more faithful nor more faithless than the rest of men. The recklessness of some contrasts with the steady attachment of others, and the bitterness of rivalry, is offset by many an instance of tender friendship between master and pupil, or between comrades of the palette. It is, too, this devotion to his art that explains the painter's indifference in matters political or even patriotic: he is at home wherever genius has left its trace. Such being the artist's character, it remains to be seen with what degree of energy he responds, when his egoism or his sympathy is aroused. Most painters are ready to make any sacrifice, to undergo any privation, that will help them to a higher niche; and this certainly means strength of will. But in point of sustained effort, such as perfect execution requires, they offer broad contrasts. There are those who go at their work calmly, patiently, toiling steadily from the first trace to the final touch—models of concentrated attention. These are the masters. Others are exhausted after the first *élan;* the sketch absorbs their warmth; the after-work finds them cold. Parallel with this difference of energy go the differing habits of life. According as we study one or the other of these classes, we see in the atelier a pattern of neatness—or a den; in the artist, precision of method and correctness in personal appearance, —or the negligent slouchy ways of a man habitually *distrait.*

Doubtless these qualities, attractive or repulsive as the case may be, go far in determining the painter's social position. But aside from individual traits, his standing in the world results from a peculiar combination of facts. His work is the work of genius, creative, ideal, and yet it must bring him his bread. The consciousness of his own worth, which never deserts him, sets him at ease with patrons who appreciate his merit, be their station what it may; but it embitters him against those who value his work at market-price, and deal with him as they would with any producer. The mercantile side of life is hard enough

for the painter; harder still the lot of those who starve in despair of the reward which they have earned but not received.

At times, misfortune touches the artist more closely; he must struggle with defects of the sense by which he lives. Some have corrected the errors to which faults of the eye exposed them; others have been misled. More serious disorders, upsetting the mental balance and ending in suicide, are not rare among painters. Even in its normal phases, genius is eccentric; but we are not, for that reason, to infer that the creative faculty is a symptom of insanity. What is exceptional in genius is the union of many happy gifts; it is the attribute of those whose works, be that what it may, "touch human chords whose vibrations are deepest."

The psychologist, after reading this volume, feels like one who has been looking through a lattice. The glimpses he catches make him regret that the view is not more continuous, that there is no central thought binding all these suggestions together. M. Arréat, as the preface declares, is far from pretending that his work is perfect. He is searching for facts, and his search has been fruitful. But when sufficient material shall have been collected, it will certainly be an interesting task to single out the traits peculiar to genius, to analyze them and reduce them, in accordance with psychological law, to their simplest elements.

PEREZ, *La maladie du pessimisme*, Rev. philosophique, 1892, XXXIII. 36.

This is a review and critique of two recent works, one by Magalhaes, *O pessimismo no ponto de vista da psychologia morbida*, and the other by Huyghe, *Des rapports de l'arthritisme avec les manifestations nerveuses.* The author advocates a psychology of diseases—a science that would connect with each morbid condition or disease its corresponding psychic manifestation. Educators would be able to diagnose the mind from the physical condition, and physicians to diagnose the body from the mental condition. A disease would have two indices instead of one. The two above-mentioned essays attempt directly, the other indirectly, to discover the pathologic condition that finds mental expression in pessimism. The one describes it as *neurosthenic affective*, the other as *arithritisme.*

Magalhaes' conclusions are based upon the study of avowed pessimists such as Leopardi, Schopenhaur, Flaubert, Baudelaire, Amiel and Byron, and of others who, without the creed of pessimism, reveal its characteristics. Among these are Carlyle, Swift, Tolstoï, de Sévancour, Shelley, and Chateaubriand.

Pessimism is regarded as a species of nerve weakness of which the chief character is nervous instability with alternation of irritability and prostration. The subject is super-sensitive; impressions call forth intense and prolonged reactions followed by exhaustion. He is characterized by a general hyperæsthesia, which naturally results in an excess of suffering. From instability and hyperæsthesia results discord between the feelings themselves,—between the feelings and the intelligence,—between the feelings, the ideas and the volitions.

The discord between the feelings shows itself in a great variety of paradoxes, contradictions and inconsistencies. To the pessimist the possession of a desired object does not atone for the former privation. The pain of unsatisfied desire is replaced by the pain of *ennui.* With inability to enjoy what he has are coupled extravagant expectations regarding that which he does not have. He is extremely susceptible, both to kindness and to contempt. He passes suddenly from violent irritability to languor, from self confidence and vanity to extreme self abasement.

His hyperæsthesia results in intellectual discords. For this involves a great vivacity of the intuitive imagination, which favors the setting

up of extravagant ideals lacking in solid representative elements. Hence a gap opens between his ideal and the actual. He can never realize the ideal he pursues and so his feelings take on a somber hue. From this excessive idealism results a mania of doubt (Amiel), a certain distrust of all his rational objective knowledge. It assumes another form in extreme subjectivism. The pessimist is haunted by images of tiniest religious scruples, suspicions, fears and anxieties, resulting in alienation from friends, seclusion misanthropy.

The pessimist is further characterized by an incapacity for prolonged attention, a refractory attention and a feeble will. These result in inaction, quietism, reverie, self-objecination, abolition of the personality, annihilation of the will, mounting sometime even to poetic or religious ecstasy.

More than Magalhaes is Dr. Huyghe concerned with the pathologic basis of pessimism. He connects it with arithritism, a constitutional disturbance of nutrition and circulation, resulting in local congestions of vitiated blood. These may result in gout, rheumatism, kidney or brain diseases. Is there any psychic aspect to these maladies? The ancients associated hypochondria or melancholia with all of them. Pessimism would seem to be the mental side of arithritism.

These essays do not ignore the existence of an impersonal, intellectual, objective pessimism, having a basis very different from that of the personal, sentimental subjective pessimism. Connecting the latter with neurosthenia or arithritism leaves the philosophic question just where it was.

E. A. Ross.

Univ. of Indiana.

Gréhant, *Sur un nouvel appareil destiné a mesurer la puissance musculaire*, Comptes rend. 1891, CXIII. 212.

M. Gréhant describes his dynamometric myograph (a modification of Prof. Marey's spring myograph) for registering and estimating muscular effort. A steel spring, $400^{mm} \times 18^{mm} \times 2^{mm}$, is fastened at one end firmly to a table, and to the other is attached an adjustable self-feeding pen, which traces, upon a revolving paper cylinder, a line of abscissa and a curve. A cord, having a wooden handle at one end, is attached to the spring at the other end. The person sits before the instrument, with his arm supported in such a position that the elbow forms a right angle, and the hand grasps the handle. By a violent jerk the forearm is bent upon the arm, and the style traces a denticulated curve. Now to measure the muscular power, it is only necessary to pass the cord over a pulley, and attach weights, until the pen traces a tangent to the summit of the curve. In several experiments M. Gréhant found the muscular power of the biceps and of the brachial anterior to be from 15 to 45^{kg}. He adds that multiplied experiments would, doubtless, yield a wider range of results.

F. Tracy.

In connection with the department of anthropology, of which Prof. F. W. Putnam is the chief, a section of psychology has been established at the World's Fair. The object of this section is to exhibit, in a typical rather than exhaustive way, the methods and results of modern psychology. The exhibit falls into two parts; the one a working laboratory for making tests of the sense powers, movements and simple mental processes, and the other an exhibit of apparatus, facilities, and results of research; the exhibits are to be labelled and made as serviceable, from a pedagogical point of view, as possible. In the working laboratory it is proposed to make tests upon visitors and to publish a statistical study of the material thus collected. In order to render

these tests as expeditious and serviceable as possible, many records are desired before the opening of the Fair, and the co-operation of all experimental psychologists is earnestly invited in the gathering of such tests. These preliminary results will furnish a norm or standard by which individuals may be classified. The final plans are not yet matured, but the interest and co-operation of all psychologists is desired. Communications should be addressed to Prof. Joseph Jastrow, University of Wisconsin, Madison, Wis., in whose charge the section of psychology has been place.

WELLS, *Clinical report of three cases of insanity of childhood*, Journal of Nerv. and Ment. Diseases, 1891, N. S. XVI. 292.

The cases of two girls (aged 15 and 11 respectively) and of one boy (aged 14) are carefully detailed. He regards the contemporaneous physical disturbances in these cases (e. g., chorea in that of the elder girl) "more as a consequence of the mental disturbance than as the causative factor of the perverted ideas." The majority of cases of child insanity fall rather under the class of idiocy or imbecility, which some maniacal excitement can accompany. In intellectual deficiency, then, Dr. W. finds the cause of most insanity in children; into the cause of the imbecility, he very properly does not go in this report.

University of Texas. WALTER LEFEVRE.

SOME POINTS IN LINGUISTIC PSYCHOLOGY.

A. F. Chamberlain, Ph. D.

Whilst lying awake at night in the region of Kootenay, British Columbia, the writer was but too often made aware of the presence of owls by their loud and expulsive hoots. After listening for some time one evening to the sonorous *tu-whit-tu-whit-tu-whu* of these nocturnal minstrels, it suddenly occurred to him to ask his Kootenay Indian companion what the owls were saying. He responded without hesitation that the owl spoke two things: (*a*) k'sētlkēnētl pátlkē; (*b*) kátskakítl pátlkē. The meaning of these phrases the writer is not able to explain as yet (but pátlkē = woman). The owl's note did not seem to vary any, although the Indian declared the owl said the two different things.

By and by the writer, without being conscious of any particular effort on his part, ceased to hear the *tu-whit-tu-whit-tu-whu*, so familiar to him, and the sounds that reached his ears were: *k'sētlkēnētl pátlkē*, *kátskakítl pátlkē*, these phrases, with the exception of the word *pátlkē*, being before entirely unknown to him, and he being ignorant of their real signification. Moreover, by a very slight effort, he was able to interchange these sounds, and to hear at will the common English or the Kootenay Indian rendering of the owl's cry. The writer took particular cognizance of the fact at the time, and when returning by the C. P. R. made the experiment of repeating words and stanzas of verse with different stress and accentuation to the series of noises made by the wheels of the cars as they reached the end of each rail. He found that, be the measure of the verses what it might, the correspondence between it and the click of the rails could still be maintained.

Mr. Cameron, of Toronto University, informed the writer, when discussing the matter, that he had noticed that similar effects were produced by experimenting in a like manner with the ticking of a clock or any noise of that kind.

The possibility of shifting from Kootenay to English in the case of the owl's cry may be susceptible of explanation in this way, partly at least. Sir Daniel Wilson[1] has recorded a similar case—it would be exactly the same if the writer's Indian had been able to hear the *tu-whit-tu-whit-tu-whu*.

"Oronyhateka, an educated Mohawk Indian, in replying to some queries addressed to him relative to his native language, thus writes me in reference to the *Caprimulgus vociferus*, or whip-poor-will: 'When I listen with my Indian ears it seems to me utterly impossible to form any other word from an imitation of its notes than *kwa-kor-yeuh*, but when I put on my English ears I hear the bird quite distinctly saying *whip-poor-will*.' Assickinack, an educated Odahwah Indian, wrote the same cry—heard nightly throughout the summer in the American forests—*wha-oo-nah*, and an Englishman, recently arrived in Canada, who listened to this cry for the first time, without being aware of the popular significance attached to it, wrote it down at my request, *eh-poo-weh*."

It is evident that a psychological study of onomatopœia would lead to some interesting and valuable results.

Some nine months ago the writer conducted a short series of experiments in this line. A series of unmeaning collections of letters (pronounced in the ordinary English fashion) were given one by one to the subject, and he was requested to state the sound which he thought was best represented by the group of letters given him. Following is the result in three cases:

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[1] Prehistoric Man, 3rd ed. (1876) II. 365.

A.

Word Used.	Sounds Thought of by Subjects.		
	Subject No. I.	Subject No. II.	Subject No. III.
Brŭv.	Dog barking.	None.	Sound made by pushing the hand or fist along a board or a piece of cloth.
Chilp.	Ring of metal; snap of thin razor blades.	None.	Chirping of a chipmunk.
Glab.	Dropping of something semi-liquid, nearer the solid than the liquid state.	Croaking of a frog.	Clapping of the hands together.
Gŏp.	Upsetting of a bowl of mush.	Sound made by a man gulping down something.	Sound made by a German drinking beer.
Hĕz.	Puffing, expelling the breath forcibly.	Sound made by a bumble-bee.	Noise made by twisting a wisp of dried hay.
Hŭth.	Same as *hez*, but more strongly.	The sound a man makes when he wishes another to keep quiet.	Hiss of a goose.
Jal.	None.	None.	One of the lower notes of an organ.
Kĭg.	Noise made in trying to suppress laughter; the gurgle of any liquid.	The sound vinegar makes running out of a barrel.	A sudden blow on something not especially hard.
Lŭs.	Striking with a "swish"; the idea of a cloth striking something and winding around it.	Hiss of a snake.	Stroke of a plane, or the stroke of a broom on the floor.
Snŏm.	A resonant sound in a lower note.	None.	Sneezing.
Splan.	A blow against a door; a blow followed by vibration.	None.	Striking two tins together.
Thrĭn.	A snapping sound, not sharp.	None.	The twang of a violin string.
Yŏz.	None.	None.	The squeak of a badly oiled vice when being screwed up.
Zĕm.	Noise made by a nail flying through the air.	Buzz of a spindle.	Sound of a buzz-saw.
Zŭt.	None.	None.	The breaking of a fiddle string.

The subjects experimented upon were members of the university, and gave their answers almost immediately.

The same three subjects were further requested to state upon what thing they would confer the name in question as being most appropriate, and also to state what word each of these meaningless groups of letters called up by its sound. The results were as follows:

B.

Word Used.	Given as a Name by Subjects.		
	Subject I.	Subject II.	Subject III.
Brŭv.	A mountain.	A brother.	A sturdy character.
Chĭlp.	A very "fresh" person.	A child.	The idea of active or brisk.
Glab.	A person who talks too much.	Foolish chatter.	Something powerful.
Gŏp.	A horse.	A "gawk"; a country fellow.	A pebble.
Hez.	A goose; a snake.	A bumble-bee.	Something waste or wild.
Hŭth.	As an adjective to something held in contempt.	A horse.	Sulkiness.
Jal.	Glass.	A girl.	A vessel for containing liquids.
Kig.	The gurgle of any liquid.	A carriage.	A dog.
Lŭsh.	Noise made in wading through water.	Wife.	A liquid.
Snŏm.	The tolling of a bell.	A cow.	A storm.
Splan.	A horse.	A plough.	A level plain.
Thrĭn.	A sleigh.	A rope.	An uproar.
Yŏz.	A bumble-bee.	A hog.	A cow.
Zĕm.	A cow.	A watch.	A house.
Zŭt.	A flash of lightning.	An axe.	A deep ravine or canyon.

C.

Word Used.	Suggested to Subject I.	Suggested to Subject II.	Suggested to Subject III.
Brŭv.	Brother.	Brother.	Brave; love.
Chĭlp.	None.	Child.	Child; Chiltern hundreds.
Glab.	Glob.	Blab.	Blab; glade.
Hĕz.	Es. (Ger.)	None.	Has; fez.
Gŏp.	Stop.	Gap.	God; got.
Hŭth.	Huff.	House.	Heath; huff.
Jal.	Jell. (v.)	Girl.	Jail; jelly.
Kĭg.	Kick.	Keg.	Kick; keg.
Lŭs.	Löss. (Ger.)	Lass.	Pus; luscious.
Snŏm.	Snob; numb.	Snob.	Snow; snot.
Splan.	Splash.	Explain.	Plan; plain.
Thrĭn.	None.	String.	Thin.
Yŏz.	Yes.	You.	Ya-a-s. (Yes.)
Zĕm.	Zim.	Gem.	Zenana; zed.
Zŭt.	Soot.	Slut.	But; slut.

Educated men of the nineteenth century may not be gifted with excessive powers of onomatopœic naming, but the short series of experiments gives hope of much better results in the future. The difference in ideas and sounds called up is considerable, and taken in connection with the great variation in onomatopœia amongst savage and uncultured races seems to demand further and more searching investigation, particularly in the direction of rhythm.

Lincoln College,
Oxford, 2 Sept. 1892.

To the Editor of the
JOURNAL OF PSYCHOLOGY:

Dear Sir—

Philosophy at Oxford forms one portion of the work required in the school of *Literæ Humaniores*, which is still the most popular of the honor schools, and contains a larger proportion of able students than any other. Until within recent years, the philosophical portion was considered the most important, and it remains of equal importance with the other subject, ancient history. The consequence is that nowhere else in England (I don't include Scotland) is there so large a body of students of philosophy. The distinctive feature of the study, as pursued at Oxford, is that which it owes to its connection with the study of classical history and literature. The statutes of the examination (and the studies of the place are regulated almost entirely by the examination) prescribe "logic and the elements of moral and political philosophy;" and in these subjects certain Greek authors are required from all students. Nominally there is a considerable choice of authors, but the course has been more or less fixed by custom. Practically every one studies the "Republic" of Plato, the "Ethics" of Aristotle, parts of Aristotle's "Organon," and Bacon's "Novum Organum." This forms the staple of the work, and as many of the great questions of philosophy are raised, in one form or another, by the Greek writers, or are suggested by contrast, these books serve as text-books upon which to base both the history of the subject and the scientific treatment of it. Students are expected to study the subject in its modern form, and they study either independently, or through lectures, some of the important English works. Thos. Mill's works are read, especially the "Logic;" the better students read parts of Lotze or Mr. Bradley's work on logic, and acquire more knowledge of the great English succession, and perhaps of Kant. "Logic," I may remark, is understood in a very loose sense as equivalent to mental philosophy, as a whole, and includes metaphysics and to a certain limited extent, psychology. On account of the importance assigned to the "Ethics" and "Republic," moral philosophy has become the most prominent part of the work and produces the best results. Political philosophy is studied in close connection with it, in such books as Aristotle's "Politics," Maine's works and the like. In moral philosophy the men read such well known works as Green's, Prof. Sedgwick's, Mr. Spencer's, Dr. Martineau's, and many read Kant's smaller work. Of course you will understand that there is some variety in the reading, and that, naturally it is, in general, only portions of these books which are read, and in Oxford, as elsewhere, the tendency is for the undergraduate to rely on his lectures to a greater extent than even the vanity of the lecturers themselves would think desirable. Besides this general course of study, special authors or books may be offered as special subjects, and sometimes are, such as Aristotle's "Politics," or "de Anima," Locke, Hume, Kant's critique and the like. Experimental psychology is one of these subjects, but has never been offered as yet, to my knowledge.

Any one who looks at the list of lectures given will be struck at first sight by two things, the great number of lecturers and the multiplication of lectures on certain subjects. These are consequences of the college system. Each college supplies a lecturer, and there is a natural tendency to lecture on the regular subjects. Including the three professors, there are as many as twenty persons giving lectures in philosophy, and in any one year there will be as many as 4 or 5 different lectures on each of the two staple books, the "Ethics" and the "Republic." All lectures are open to the whole University, but there is not

much organization for division of the labor. It would be tedious to enumerate all the subjects usually given in lectures. Prof. Wm. Wilson lectures at present on systematic logic, sometimes on Aristotle's logic. Prof. Wallace lectures on the history of ethical theories, and on other ethical subjects. Prof. Case on Aristotle's metaphysics, and on Aristotle's and Bacon's logic, and on general metaphysical questions. There are other lectures on logic, on Bacon and Aristotle, on the English philosophers of the 17th and 18th centuries, elementary metaphysics, philosophical terms, on the English moralists, on Kant's ethics, and on moral and political philosophy as a whole. Mr. D. G. Ritchie, of Jesus College, is one of the most popular lecturers on political philosophy. I will not mention the names of many of the lecturers. One of the most influential has been Mr. R. L. Nettleship, of Christs, the editor of Green's works, who has just been taken from us by an untimely and melancholy death. Greek philosophy offers a field for many distinguished scholars, like Prof. Jewett, Mr. Bywater, Mr. J. A. Stewart. Writing for a journal of psychology, I have to confess with regret that systematic lectures on psychology are infrequent. Wm. Blunt of Christchurch has lectured on the subject, and I now give a course of lectures, and hope to extend activity in this direction. To some extent psychology is treated in the lectures on logic. But the subject of psychology is the most glaring weakness of the Oxford teaching of philosophy.

The lecture list gives a very imperfect idea of the system of teaching, a large part of which is done privately by the college tutors. It is the custom for each student to bring a written essay on some philosophical subject to his tutor at stated times, generally once a week during certain terms. These meetings give the tutor an opportunity of discussing questions thoroughly with his pupils, and at the same time of suggesting courses of reading to suit their special tastes and aptitudes. Many persons think this the most valuable part of the training, and it is certainly the most characteristic feature of the Oxford system. Others, while admitting its value in many cases, think it wasteful to the energies of the teacher, without producing a proportionate advantage to the taught. Any one who has seen the large amount of solid and able work which is produced in the examination, must admit that the teaching, whether by public lectures or private tuition, is highly effective.

But the demands made by the examination on the men are so great (for they have to read Greek and Roman history as well as the original writers) that there is necessarily little room left except in the case of the ablest men for originality to show itself.

It is not very easy to say what the prevalent character of the teaching in philosophy is. The lecturers do not often try to profit by attending each other's lectures, and one has to guess from the best produced by the men in examination, which does not at present indicate any very dominant mode of thinking amongst the lecturers. Until recently the influence of T. H. Green was very marked, and on the whole this philosophy is still the strongest. But, the spell of Green's personality removed, the idealistic German philosophy by which Oxford has been so deeply influenced has lost much of its potency, must I say for evil or for good? In general the atmosphere is more favorable to criticism than to construction. An outsider might justly charge the University with what Mr. Spencer calls the anti-patriotic bias in philosophy. We have been so occupied with the defects of our homebred philosophy that we have neglected its merits. I should mention, while speaking of the character of the teaching, that our most eminent resident worker in philosophy and psychology, Mr. F. H. Bradley, has never taught in lectures, and another eminent writer who used to be a lecturer, Mr. Bosanquet, lectures at Oxford no longer.

The list of lectures from which I have quoted shows a considerable variety of subjects which are lectured upon. And, of course, it gives no clue to the special private studies of individual teachers, and to the possibilities of special guidance which a student would be sure to find from some one teacher or other, amongst so many engaged on the subject. Still the connection of philosophy with other subjects like ancient history, while it has peculiar merits and is remarkaby successful in its results, makes it difficult to study the science as a whole very fully, or its more special department in any detail at all. There are at present no post-graduate courses such as you have in the United States. Perhaps greater opportunities will be given in the future for those who wish to study the subject without giving too much attention to Greek authorities. A full recognition of the claims of psychology is especially to be desired. It is a welcome fact, at the present term, that the younger graduates are turning more and more to this subject. Before long we may, I trust, have a laboratory for experimental psychology. It is not too much to hope that the university of Hobbes and Locke may once more contribute by active teaching and active study to that philosophical subject which is most closely associated with the name of England. Believe me

Yours very truly,

S. ALEXANDER.

THE AMERICAN

JOURNAL OF PSYCHOLOGY

VOL. V NOVEMBER, 1892. No. 2

ON THE DEVELOPMENT OF VOLUNTARY MOTOR ABILITY.

WM. L. BRYAN.

PREFACE

On the Requirements of Work in Experimental Psychology.

I.

Work in Experimental Psychology must meet two requirements. It must be carried out according to the best attained methods of scientific research; and its results must contribute something to the knowledge of conscious life. The latter requirement is sometimes expanded to mean that the contribution must throw effective light upon general problems of Psychology and Philosophy; and sometimes it is expanded to mean that the contribution must be of some practical use.

II.

The Experimental Psychologist has no choice and no wish for choice against the requirement for exact method. It is true, indeed, and needs emphasis, that in the present state of Psychology a vast deal is to be hoped in certain fields from very simple methods, intelligently planned. More exactness than the subject requires is pedantry and waste. It is a fundamental error to suppose that the same exactness in experiment, and the same strictness in deduction, are possible or useful in all scientific work. It is a principle of wide application that degree of system in procedure should correspond to the degree of system in the material and situation

to be dealt with. To establish in a logging camp, the governmental machinery which is found essential at Washington, to keep debit and credit for a kitchen garden with the system of books used in the New York Clearing House, to enforce at a fox-drive the discipline of the German army, would be ludicrous violations of the principle. But a multitude of examples no less ludicrous appear in the history of science. Systematic methodology has, hitherto, almost wholly failed to recognize that science is subject to the law of evolution,—*exists* in all degrees of development. The constantly recurring delusion has been, that processes found fruitful in the more highly developed and exact fields of science, may profitably be applied to all phenomena whatever. As a matter of fact, the "inexact sciences" are only burdened by "scholastische Zahlenhaufen" (Münsterberg's apt word), which are more precise than significant, or by strict deductions, whose strictness can be only in words. We need a logic based upon the historical development of science to set forth the whole law in this matter. Meanwhile the individual investigator must be a law unto himself.

At the same time it must not be forgotten that Physiological Psychology is not so new as its name. It is historically a special outgrowth of older sciences. Its oldest classics are the work of men trained in Physiological research. The modern Psychologist, accordingly, fails to find in his specialty a paradise of windfalls. He acquires, instead, a practical realization of Comte's generalization that the more complex science presupposes and works by means of the other sciences. If there were any such thing as a perfectly trained Experimental Psychologist, he must have had thorough apprenticeship not only in the results, but also in the technique of Chemistry, Physics, Morphology, Physiology and Mathematics. Or since no one man can have all these knowledges and skills, the body of Psychologists must have them. The combined results of human ingenuity in every direction may and must be made to bear upon the elusive problems of conscious life. Such concentration of all available forces upon the problem in hand is the ideal of every Psychological research. To do this, at least in the measure attained by works of standard excellence in the same field, is a requirement not to be escaped.

III.

The demand for results of general significance requires consideration. Perhaps no one has ever been more urgently called upon to consider this demand than the Experimental Psychologist. For, perhaps no one has been more urgently

solicited from within and from without to "keep his feet upon the earth and yet to carry his head among the stars." He is called upon to be both scientist and philosopher in one. As a matter of practical attitude, at any rate, every Psychologist must give the demand for general results some kind of answer.

This demand seems to be eminently rational. For progressive organization seems to be a universal character of growth. All evolution, organic and inorganic, conscious and unconscious, individual and racial, appears to involve advance of the relatively isolated into more perfect unity. The main occupation of every living thing seems to be the transformation of a relatively chaotic environment into stuff of finer organization; and degree of organizing capacity seems to be one fair measure of a being's place in nature.

One who has arrived at this conclusion, with whatever arguments therefor, is rarely undecided in his judgment of the relative value of concrete and general scientific results. The concrete scientific result, standing in no obvious relation to any general law, is not, he may allow, without value; but its value diminishes the more concrete and isolated it is. In the presence of the greater generalizations of science, such unrationalized bits of knowledge seem to him trifling; in the presence of the insights of philosophy, they become practically insignificant. From men who have this view, the demand upon Experimental Psychology for general results is particularly imperative. "Do your experiments throw light upon the nature of the soul?" they ask. "Do your statistics determine a system of Psychology? Does your delicate machine enable you to establish, or disestablish, any general law of mind? If not, if you have nothing to show but an uncorrelated fragment of information about the conscious life of infusoria, or about the knee jerk, or about the time in which you can wag your finger, all this may be very exact, but it is almost impertinent to call it a contribution to Psychology and it is altogether folly to call it a contribution to Philosophy."

What answer can be made?

1. In the first place, it is submitted, subject to the facts of the History of Science and Philosophy, that reaction against established general theories toward concrete reinvestigation from the ground up, has justified itself as an essential part of the process of intellectual growth. "Jeder tüchtiger Denker ist zuerst Zweifler," says Herbart. One must have studied the lives of the most productive men of history very superficially indeed not to see that this is so,—that the resolute skepticism, negation and descent which, for example, Des

Cartes describes in himself, must prepare the way for the more obviously profitable creative work.

The same thing appears no less in social Psychology. Mr. William T. Harris has observed, as one of the mysterious phases of the History of Philosophy, that the "shallow thought of Nominalism should have triumphed for a long period over the deeper and truer thought" of St. Thomas Aquinas and the orthodox theology. He offers in explanation of this mystery the consideration that the "deeper and truer thought," although it "emancipates humanity at first, after a time imposes on the soul a sort of external authority and needs to be replaced by a newer freedom." . . . . . "It is wonderful," he says, "to see how the most negative phases, the skepticisms, the heretical doctrines, the most revolutionary phases in history . . . . are destructive only in their undeveloped state and when partially understood. By and by they are drawn within the great positive movement, and we see how useful they are become."

Suppose now the results of Psychological research were as fragmentary and unmeaning as the most ignorant critic believes or hopes, how should we account historically for the social movement which has produced them? How could men who sat at the feet of Kant, Hegel, Herbart, and other such masters, turn away to these husks?

Might we not make a beginning of answer with the foregoing explanation of spiritual revolt? Might we not say that the modern movement in Psychology constitutes a protest against the final sufficiency of the howsoever superior systems which the world already possessed? If this movement were wholly negative, heretical and destructive, might we not expect that it would "by and by be drawn within the great positive movement" toward a philosophy, through it more rational and emancipating than those it forsakes.

No such revolt is nihilistic. It is essentially an appeal from the schematized reason of the books to the finer reason of reality. It is an expression of that saving discontent which drives men always from the good toward the everlasting better. To regard such a movement as a descent is an altogether distorted view. If it be descent, it is the descent in search for solider foundations. If we dig down, it is that we may build the higher.

2. We have, however, a far more fundamental justification of concrete Psychological research when we view it as —what it is intended to be—a contribution to a long cooperative task. This way of looking at individual work is very familiar in Philosophy and Higher Anthropology. From many sources ancient and modern, scientific and phil-

osophic, from Aristotle, Leibnitz, Hegel, Darwin and Spencer, we have some more or less explicit statement of this view. Human life in this world is regarded as a development, to which every man's individual accomplishment is a more or less important contribution. Therewith, conscious activities and attainments—whether of child, savage, average civilized man, poet, saint or philosopher—are made to appear as stadia in the growth of mind. We have, accordingly, at bottom the view of *a race working together*, consciously and unconsciously, by force of circumstances, by instinct, or by intelligent purpose, through the long task of comprehending the world.

This view carries with it two direct implications. The first is that *no* sort of intellectual effort is quite without justification. The ideas of childhood and of the childhood of our race, the myths, cosmogonies, and grotesque theologies, as well as the scientist's fact and the philosopher's generalization—who shall say that any one of these has contributed nothing to the development of culture? To despise the study of the conscious life of a spider is unphilosophic. To despise the study of the conscious life of Plato is unscientific. To be dismayed at the world full of warring ideas as though they threatened the Sovereignty of Reason is unintelligence and lack of faith. These derisions and fears are no doubt natural—to minds of a certain development—but they disappear from every mature and reverent view of the world. The second implication of this view is that intellectual values are not equal. It is not a theory of indifferentism. On the contrary, growing out of this view, or in essential harmony with it, is a universally recognized standard for valuing intellectual work, namely, how much has the work contributed toward accomplishing the intellectual task of the race?

Now it is a singular fact that while a philosopher is much the more apt to recognize this view in theory, a scientist is much the more apt to realize it in practice. The philosopher is very much inclined to limit the application of the theory to his predecessors and contemporaries. The instinct of the philosopher is to complete by himself, in the general, the intellectual task of the race. Very often he believes he has done so. In his system, the long intellectval evolution has culminated.[1]

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[1]For illustration of this tendency in the philosopher, one thinks perhaps most readily of Hegel, whose exposition of the History of Philosophy as a necessary evolution, wherein individual systems are necessary successive stadia, is not felt by him to be inconsistent with the claim that his own system is a culmination of the evolution, a finally valid general view of all reality.

A not less interesting illustration of the same tendency is to be found

Quite otherwise the scientist. The Experimental Psychologist, for example, may or may not have a Philosophy of History wherein every man's work, including his own, is regarded as a small contribution to a social task. He may not have the theory, but he does the thing. From inclination or from resolution he foregoes the making of a system, and tries to furnish some material for one. He is cheerfully willing to fix one point and drive a peg down there, whether any other peg is in sight or not. He makes no apology for his uncorrelated fact. He denies the right of the present to determine its final value. He leaves it for the justification which time shall show. He is willing, in short, to make one in a vast social endeavor instead of trying to complete the whole task by himself.

The foregoing is intended to be a defense of concrete Psychological research, even when the results throw no immediate light upon general theories of life and mind. It has been for the time conceded that the results in Experimental Psychology are all of this character. The concession was, however, only temporary. It is flat ignorance to suppose that the body of Psychologists are working without intelligent aims, somewhat, for example, as the earth-worms, which contribute to civilization without intending to do so. It is true, indeed, that there are men in this as in other fields of science who profess horror of generalizations. When the History of Science is written from the Psychological standpoint, the etiology and uses of this type will no doubt be made to appear. It may be that those often skilful and productive scientists, whose fear of generalizations amounts to a phobia, represent the extreme swing of the pendulum from the other extreme of reckless speculation. They perhaps exhibit, in the social scientific movement, in a large and obvious way, that period of skepticism, negation and blind groping which the individual thinker generally passes through on his way to a more obviously productive period.

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in that present day philosopher who is popularly supposed to be the special champion of empirical science and the special foe of dogmatic philosophy. Mr. Herbert Spencer presents a system of First Principles, of which it is affirmed:

1. That Mr. Spencer is the first in the course of evolution fully to realize them. 2. That they are strictly deducible from an ultimate principle, which permits empirical illustration, but which does not permit empirical proof. 3. That they hold good for the whole and for every part of every one of an infinite number of successive epochs of world-evolution and dissolution. How it can be so confidently foreknown that the Unknowable—in which all things, changes and laws have their being—contains no potential modification of Mr. Spencer's valuable generalization, is not known to the writer.

Such periods are probably necessary to the philosopher, and such men are useful, negatively and positively, in the development of science, even if they generally are as they have been aptly called, "die Handwerker der Wissenschaft."

The leaders in Psychological research are, however, not at all of this description. The ignorant derision and neglect which so often express the attitude of the immature mind toward philosophy, find no sympathy from any recognized master in modern Psychology. It is, on the contrary, a fact that the leaders in this movement are not only acknowledged masters in experimental science but thoroughly schooled in philosophic disciplines. Munk has expressed the ideal in speaking of Helmholtz: "*Bei der genauesten Ermittelung des Einzelnen, das Ganze nicht aus dem Auge zu verliern; bei der Erwägung des Allgemeinen immer wieder Kraft und Sicherheit am Besonderen zu erproben*" ([1]). It is an inestimable good fortune that this is so. For a social movement, howsoever much it may be beyond the precise control of any man, is nevertheless even as other force, largely directed by the men who best understand it.

Under such leadership, the rapidly growing company of Psychologists have learned to take no narrow view of their common task. That study of the development of the unconscious world whose results make up most of what goes under the name of modern science, Psychology will supplement by a study of the development of conscious life, from its darkest beginnings to its apotheosis in science, philosophy and religion. This work, it is profoundly believed, cannot be forestalled by general laws, however obtained and however true. We cannot dispense with organized mortality statistics, because we know that all men are mortal. The most ingenious philosophic reflection cannot anticipate the special phenomena of human activity and the special laws which they will reveal. There is no device for avoiding the task which the actual, finite and definable interactions between individuals and society make possible and imperative. "Die Höhe reizt uns, nicht die Stufen" Gœthe's Wilhelm Meister is told. But the *Stufen* are not to be escaped. It is, therefore, evident that this must be, in the broadest meaning, a co-operative task. Money from men of wealth, or from all the people through the state; the experience and manual skill attained in the mechanic arts; the instruments, devices and discoveries of the older sciences; the suggestions of Philosophy back to the earliest myths; the divinations of art and religion; men able to plan, and men willing to work; all the forces in co-operative civilization must come together for the making of the Science of Psychology.

IV.

The demand for practical results, which has every meaning that varying stages of culture give to the word practical, has already received an implicit answer. As the most profound philosophy is most cautious against premature philosophizing, so the highest practical sagacity is least inclined to force premature practical results. The most impractical requirement upon science is to limit it to a search for alleged practical results. Wise men do not demand loaves from corn in the blade. Science must be allowed to develop in freedom and bring forth fruit in its season. If what is true makes the best direction for what to do, we may be sure that every truth found will prove itself practical in more and better ways than anyone has thought of hoping for.

Here again, however, Psychologists are not working without aim. The most practical questions, the questions in which men generally are most intensely interested—the questions of health, education, government and religion, which are deepest in the conscious and unconscious life of the world,—these are the questions for which modern Psychology is gathering force. Sagacious men are saying that the next years are to be the Psychological Epoch. There are, at any rate, abundant signs that that intense public interest in science which is always interest in man, even when it is fixed apparently upon some law of Copernicus, Lyell or Darwin, will be challenged next by the results of Psychological research. And it is fair to hope that this will be a schooling no less beneficial than the former ones have been.

The Development of Voluntary Motor Ability.

I.

In studies which involve the development of will, it has usually been thought necessary to begin or at least to conclude with a theory of the source of the force appearing in voluntary motion. This appears not only in works avowedly metaphysical, but scarcely less in works avowedly anti-metaphysical. This is so evident even in strenuously agnostic writings, that metaphysicians may very well appeal thereto as a profound historico-psychological justification of their own occupation.

Concerning the relation between special investigation and metaphysical postulates, explicit or implicit, nothing will here be said. But it is at any rate undeniable that investigators whose *explicit* metaphysical postulates are contradictory, make contributions to a common fund of knowledge. That this is the case even in the study of the development of

will in the individual or in the History of Culture, is a historical fact. How this can be so may appear from the following:

It is evident from the phenomena of growth and it is generally agreed that the activities of a living organism are determined at any point in its history, partly by influences from its environment and partly by the subjective constitution of the organism at that point in its history.

This generalization leaves open the question whether any part of the "subjective contribution" is essentially innate and independent of the rest of nature, or whether all subjective energies have been taken in from the environment and stored up by the individual and its ancestors. Instead of a solution of this metaphysical[1] dilemma, we have in the generalization only the outlines of a scientific task. Whether the metaphysical problem be solved or not, and however it might be solved, the scientific task remains the same. It is the task upon which all students of the growth of living things are in some way engaged.[2]

The study of the development of will in the individual as well as what may be called the development of the "social will" in the History of Culture may be regarded—must be regarded as part of this task. We have, namely, the conditions of a vast experiment. We have on the one hand the world of forces by which the activity and growth of the will are modifiable; an infinite range of things from barometric pressure and cookery, to educational systems and the Spirit of the Times. On the other hand, we have the fact that even the simplest neural reaction is not a simple reflection of the stimulus applied; but that cerebrum, spinal cord, or nerve-muscle machine, each in some degree contributes—from whatever ultimate source—to determine the resulting reaction, and so exhibits something of its own constitution. Every action of animal or man, whether elicited by the "natural" events of life, or by the device of the experimenter, becomes accordingly a source of information about the existing constitution of the subject. It is obvious that we have in this way a general method for studying individual and social development. The same kind of experimentation which shows what

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[1]By metaphysical I mean to describe the knowledge men are held to possess of the absolute nature and source of things. I call this a metaphysical dilemma because its solution seems possible only from a knowledge of the absolute nature and source of the force appearing in the organism.

[2]Among Morphologists, one finds the same dilemma, in the form of a dispute whether or not the embryo has any innate "formative force." Meanwhile men of both views work productively side by side.

the spinal cord can do, shows how much more the cerebrum can do. The same general method of observation which shows the capacity of a child, can follow the enlarging capacity of the child to modify his environment and to shape his own course therein. From the observed reactions or results of reaction of men upon the world, it is possible to write that history of human emancipation, which we call the History of Culture. We have thus a standpoint which leaves open every question as to the absolute nature and source of the forces appearing in action and which, nevertheless, permits the study of the will through every stage of its development, from the events to which the subject contributes.

In gaining this scientific point of view for the study of will, we have at the same time gained a reason for the study of voluntary muscular motion. For to the Psychologist or Sociologist, it can not be an insignificant fact that:

"L'infinie diversité des manifestations extérieures de l'activité cérébrale,............................l' hilarité de l'enfant à la vue d'un jouet, le sourire de Garibaldi persécuté pour avoir trop aimé son pays, le tressaillement de la jeune fille à la première pensée d'amour, l'énonciation verbale des lois de Newton ....toutes les manifestations extérieures de l'activité cérébrale se réduisent aux mouvements musculaires." (2)

But the bare fact that all, even the highest, immediate manifestations of the mind are muscular motions does not at once make apparent the deeper justification for the study of those motions in Psychology and the History of Culture. The fact alleged is denied by no one. But many would hold that the motions involved in signing the Emancipation Proclamation, and those which a child might make with the same pen are so different in every respect which concerns Psychology or the History of Culture that their undoubted points of likeness may be neglected as trivial. It is, therefore, necessary to recall the fact that the activities which make up what is called the higher life of humanity are not isolated, but have inescapable connections with the activities and achievements which are usually called lower. It is seen, for example, by discerning men that the development of art, science, philosophy, political institutions—of all that goes under the name of cultivated life, has been made possible, in a large degree, by "material civilization." The more extensively and the more intensively the History of Culture is studied, the more does this historical dependence appear. The progressive attainment of material wealth is necessarily accompanied, in a somewhat corresponding degree, (1) by an increased knowledge of the laws of nature, (2) by an increased amount of force at disposal and an increased skill in its

manipulation, and (3) in consequence, by an increased freedom from the control of the immediate environment.

That the *outer* conditions essential to the development of higher culture are furnished in this way is evident from all historical study, even if it were not generally realized in the personal experience of men devoted to any form of cultivated life. Any work requiring leisure must have the leisure provided by some form of stored work. It is practically impossible for art or science to flourish except by help of the stored work, which material civilization has provided. "Before we can live well, we must manage to live."

It is less evident but more, rather than less important that the *inner* conditions of higher culture are prepared by the struggle for material wealth. The earth does not give up its wealth without teaching something of its laws. This knowledge may not be so extensive, so precise or so well organized as that which we at present call scientific. But it has one high mark of truth. It works. Which means that within important limits it is true. Moreover, if not so extensive as science, it is generally more intensive. It is less knowledge than practical wisdom. Such as it is, it is the subsoil out of which all higher forms of culture grow.

To a sufficiently superficial view, the most essential requisite of this industrial civilization appears to be machinery, for it is only by machinery that men are able to control indefinitely great force with indefinitely great precision, and so to gain their indefinitely great ascendency in the world. The smallest penetration shows, however, that the one essential machine by which all other machines have been made, and for which all other machines are supplements, is the nerve-muscle apparatus. The bare-handed man has at disposal comparatively little force. He can manipulate this force with comparatively little precision, either in space, in time or in intensity. He has accordingly comparatively little freedom; comparatively small ability to modify his environment and to help determine his own course therein. Much or little, however, this power and skill and consequent freedom are the fundamental capital of life. All greater powers through machines, all finer skills, through instruments of precision, all larger freedoms up to the highest which men enjoy, have their essential pre-condition and their prototype in the howsoever modest attainments made in the individual body. When, besides, it is remembered that the life-long and world-long expression of thoughts and feelings solely through muscular movements makes muscle habits infinitely the most subtle and complete record of the conscious life, and when it is remembered, further, that these muscle habits constantly react with deter-

mining power upon the whole activity and growth of the mind, it is not enough to say that the subject is entitled to study from the standpoint of Psychology and the History of Culture. It must rather be said that these sciences will be obliged to study the development of motor ability. We have a right to expect from such study a typical chapter in the whole progress of man, a "grammar of will." And we have therefore a right to hope from such study new and fundamental approaches to the understanding of the History of Culture.

Inasmuch as voluntary motions are data at once for Physiology, Psychology and the History of Culture, one might expect to find that they had already received attentive study from all, or at least from some, of these standpoints. In fact, however, the point of common interest has been a point of common neglect. Students of the History of Culture have found overwhelmingly abundant material for research and speculation in the results,—the records of human reaction upon the world—tools, buildings, works of art, languages, books, rites, governments, etc. They have accordingly been able to overlook the muscular motions through which alone these "works" have arisen. The manifest importance of the sociological material, moreover, has made muscular motions, as such, seem comparatively elementary and trivial. It may accordingly be understood why it is only in rare instances — as in one department (phonetics) of the old and highly developed science of philology — that we have any considerable study of muscular motion.

If the study of voluntary motion has been postponed in Sociology because of its simplicity, it has been postponed in Physiology for just the opposite reason. "Man kann behaupten," says Fick, "dass die ganze thierische Organization jene ('willkürliche') Bewegungen zum Zwecke hat" (35). But of the "riesiges Material von Versuchen über Muskelzusammenziehung," of which he speaks (p. 2), nearly all is engaged with the more elementary phenomena of nerve-muscle action rather than with what von Kries calls "resultirende Bewegungen." Von Kries says (3):

"Einer Untersuchung der willkürlichen Muskelthätigkeit bieten sich wesentlich zwei vershiedene Aufgaben, welche, wiewohl in naher Beziehung zu einander, doch sorgfaltig unterschieden werden müssen. Wir können zunachst die Bewegungen beobachten welche sich an den festen (knöchernen) Theilen des Körpers durch willkürliche Muskelthätigkeit hervorbringen lassen; wir wollen sie kurz die resultirenden Bewegungen nennen. Da es shon bekannt ist, dass diese

Bewegungen in der Regel durch ein verwickeltes Zusammenwirken vieler Muskeln bewirkt werden, so erhebt sich als weitere Frage die nach der Thätigkeit der einzelnen Muskeln. In dieser letzen Hinsicht steht seit geraumer Zeit die Frage nach gewissen zeitlichen Verhältnissen insbesondere nach der Stetigkeit oder Discontinuität, eventuell nach dem Rhythmus der Innervation, im Mitteltumkt bes Interesses, ohne jedoch bis jetzt abschliessend beantwortet zu sein. Aber auch in der ersteren bieten sich gewisse einigermaassen ähnliche Fragen welche mir ein selbständiges Interesse zu verdienen scheinen."

The efficiency of a machine depends, so far as we know, upon the maximum force, rate, amplitude, and variety of direction of its movements; and upon the exactness with which, below these maxima, the force, rate, amplitude and direction of its movements can be controlled. The motor efficiency of a man depends upon his ability in all these respects. All of them are determinable within varying limits of precision. All of them have been made objects of research more or less limited. But an adequate determination in respect to any of them, either for the average adult, for children at successive stages of development, or for the sick or aged in in successive stages of decline, is wanting.

The following research deals with the development of voluntary motor ability of children with respect to

1. The maximum rate of rhythmically repeated movement.
2. The precision of voluntary movement, particularly as regards direction and force.
3. With a note on the bilateral development of strength and endurance.

The Maximum Rate of Voluntary Movement.

LITERATURE.

1. *The maximum rate of inervation.*

The maximum rate of inervation has been reported differently as follows:

Helmholtz(4), 1866,		18–20 per second.
Hall and Kronecker(5), 1879,	about 20	" "
Horsley and Schaefer(6), 1886,	10	" "
Schaefer, Carney and Turnstall(7), 1886,	8–12,	Av. 10 per sec.
Von Kries(3), 1886,		11–12.4 per second.
Griffiths(8), 1888,		8–21 per second (see below).
Haycroft(9), 1890,	about 19.5	" "

H. thinks that the muscle vibrations "cause and compound themselves" with rhythms in the instruments used, and so endeavors to explain former contradictory results.

2. *The Maximum Rate of "Resulting Movements."*

Von Kries(7).

Shortest movement of middle finger (Av. 11 trials), .077.
Hand, (Av. 10 trials), .074.
Tongue, .066.
Foot (plantar flexion), .125″–111″.
Jaw, 125″–111″.
Maximum rate of rhythmically repeated movement after practice, 10–11 per second.
Vocal organs about same as hand.
Respiration in dogs has been observed at 7 per second.
Cattel and Fullerton(10–1892), for a movement of 50 cm. time varies from 87″ to 118″ in four individuals.
Dresslar(11–1892) (when 300 taps were made), 6.5–10.5 per second.
For a short time, 11 per second.

Dresslar gives the records of 27 adults. The average of these records is about 6 per second; 300 taps were made in each case.

3. *Influences Affecting the Rate of Movement.*

Horsley and Schaefer(5). The rate of the muscle rhythm is the same when cortex or spinal centers are electrically stimulated and when the muscle is voluntarily stimulated.

Griffiths(8). The rate of voluntary muscle rhythm varies in different individuals, different muscles, and with fatigue.

Dresslar(11). Muscular exercise lowers the rate; mental excitement increases the rate. There is a daily rhythm with the rise and fall of mental and nervous tension.

Cattell and Fullerton(10). Women have decidedly slower rate than men. The rate is very constant.

Von Kries(7). The rate varies slightly in different muscles; increases with practice; but is not affected by variation of the amplitude of motion within wide limits, a certain medium amplitude requiring less time than longer or shorter distances.

(See v. Kries' tables (op. cit. p. 4), where it is shown that excursions of 10 mm. and 16 mm. are made by the middle finger in less time than are excursions of 4 mm.; that excursions of 19 mm. and 25 mm. are made in less time by the hand than are excursions of 9 mm.; and that a decided increase in time over that required for excursions of 4 mm. or 5 mm. does not appear in excursions of less than 30 mm.)

Camerer(12), 1866.

The will brings about an intended rate of movement only gradually. Constant rate of motion is unnatural and forced. The natural rate of motion is one of constant acceleration.

Conclusions from the Literature.

The maximum rate of voluntary muscle rhythm is not satisfactorily determined.

The maximum rate of voluntary rhythmically repeated "resulting movements" in adults has been found in some cases 11–12 per second. The average maximum rate of adults is not determined.

The maximum rate varies with individuals, with muscles used, with fatigue, and with mental excitement; but not within wide limits, with the amplitude of the movement.

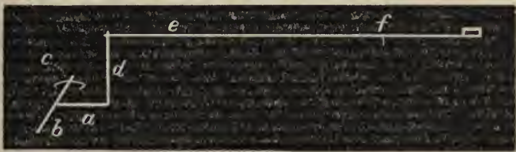

FIG. 1.—Scheme of Mechanical Counter.

Apparatus (see Fig. 1). A spring clock movement (cost $1.00) was taken out of its case. The balance wheel with its spring was removed. The axle (b) which supports the escapement lever (c) had rigidly attached to it, and extending about four mm. horizontally from it, and at right angles to it, a strip of brass (a). The outer end of this strip was fastened to a light wooden lever (e). The attachment between the two was made by means of another strip of brass (d) about 5 mm. long, fastened to each by fine flexible wire loosely enough to allow necessary play. The fulcrum (f) of the lever, distant about 34 mm. from the clock, was supplied by a Morse key, the arm of the key forming a rigid continuation of the lever attached to the clock. The key and the clock were each firmly fastened to the same wooden base at a distance determined by the length of the lever. The end of the lever attached to the clock, was made to occupy a position directly above its point of attachment, at a distance determined by the length of the connecting strip. This distance demands rather delicate adjustment, in order that the upward and downward movements of the lever may cause a properly balanced upward and downward movement of the escapement lever. When proper adjustment is made, taps upon the button of the Morse key permit the 'scape wheel to revolve, one cog and only one, for each tap. To prevent possible errors in the train of wheels connecting the escapement and the second hand, a hand was attached to the axle of a second's wheel, which engages directly with the 'scape wheel, the clock face being secured in a corresponding position. It requires 120 taps to permit the hand to make one revolution.[1]

[1]There are 48 cogs on the second's wheel engaging with 6 cogs on the axle of the 'scape wheel. There must therefore be eight revolutions of the 'scape wheel to permit one revolution of the second's wheel. And since there are 15 cogs on the 'scape wheel, there must be 8 x 15 escapements in order that the second's wheel may revolve once.

The distance through which the button of the Morse key must be moved depends of course upon the length of the lever employed. The maximum vertical movement of either pallet of the escapement lever, in the clock used, is 1.5 mm. The depth of the cogs of the 'scape wheel is 1.1 mm. The distance from weight to fulcrum, and from fulcrum to power, being 34 mm. and 9 mm. respectively, the minimum and maximum movements at P must be respectively .29 mm. and .39 mm. The amount of force required is insignificant.

The accuracy of the apparatus was tested by placing the Morse key in a circuit with a Duprez signal, in position to write upon the kymograph. It was found in many trials that the number of taps recorded upon the clock face corresponded with the number of taps recorded upon the drum. An (undescribed) apparatus devised by Dr. E. C. Sanford, differing from that described in that the lever is attached to the armature of a magnet and moved to and fro by the counter action of an electric current and spring, was tested by the Kronecker interrupter and proved correct to 20 counts per second. As the two devices are essentially the same, except in the mode of moving the lever, this test indicates the probable capacity of the instrument used by me. A few of the measurements hereafter described were made with Dr. Sanford's apparatus.[1]

The Course of the Experiments.

All the joints of the upper extremities were tested. The conditions of the tests were as follows:

Shoulder.

The forearm being held at right angles to the upper arm, and the back of the elbow being held in position above the button of the Morse key, the upper arm was caused to move up and down as rapidly as possible in a plane nearly parallel with the vertical plane of the body, i. e., in about that plane in which the arm tends to swing when one walks.

Elbow.

The elbow rested upon the table. The forearm was held at right angles to the upper arm. The key was struck with

[1]The use of reaction time as a general clinical test has, it is well known, been made difficult by the cost of apparatus and by the delicate manipulation required to secure reliable results. If it should prove true, as now seems probable, that the rate of voluntary movement is a valuable supplement to the reaction time test—if not also in many cases a good substitute therefor—these difficulties will not be met. The apparatus which I have described, for example, can be made at a cost of two or three dollars and will give reliable results without more time or care in manipulation than many clinical tests in general use require.

the ventral side of the forearm just back of the wrist. In a few instances the arm was extended and while the elbow rested upon the table, as before, the taps were made with the little-finger side of the wrist. No difference appeared in the rate of the two motions. It is to be noted that the school experiments were made with the ordinary (Spring) clothing about the arm. In my own case, the removal of my coat made no discernible difference in the rate of elbow or shoulder, owing, I think, to the great disproportion between the force of the limb in movement and the resistance which ordinary clothing presents.

Wrist.

The elbow rested upon the table. An iron clamp, whose jaws were covered with firm cushions of cloth over cotton, was placed rather loosely about the forearm just back of the wrist joint, and was held in position by an ordinary stative. The key was struck with the palm of the hand.

Metacarpo-Phalangeal Joint of Forefinger.

The palm of the hand was held with moderate firmness at an angle of about 135° with the forearm. The finger in position was then nearly or quite parallel with the forearm. In this way possible sympathetic movements of the wrist were prevented from affecting the record. If properly placed, the clamp does not interfere in the least with free movement of the finger. Other phalangeal joints were tested only in the case of adults. A narrow clamp was then used and, as in the case of the hand, interference with other joints was prevented in part by the position in which the member was held.

In all the experiments reported in this paper, the maximum number of taps in five seconds was determined, and all results are given in terms of x taps in five seconds. A stop watch, or rather a timer, measuring fifths of a second, was used to measure the time. In 60 seconds this timer shows no discernible variation from a standard second's pendulum. For measuring periods of five seconds, therefore, the readings do not differ from those which could be obtained from a theoretically perfect instrument.

In the case of adults, the subject, being in proper position with reference to the tapping apparatus, and with the timer before him, began to tap as the watch hand passed a five-seconds mark upon the dial, and ceased tapping as the hand passed the next five-seconds mark. It is obvious that each time, the personal errors at the beginning and at the end of the interval tend to balance each other, and that, in the long run, the plus and minus errors in this balance tend to balance each other. In the school experiments the starting and stopping followed a word of command. The timer was

started as nearly as possible at the same instant in which movement was observed to begin. When five seconds had elapsed the command to stop was given and any subsequent tap was not counted. The reaction time of the pupil was thus not included, and the observer's errors tend to balance as above. The error from this source can scarcely exceed one tap in a single test.

To prevent incipient fatigue, slight pauses were made between each five-second period of work, with longer pauses every second or third time.

All the rate tests were taken by myself, except possibly a dozen taken by my wife, who assisted me throughout every part of the present research, and who was thoroughly familiar with every detail of the work.

Preliminary Experiments.

Besides furnishing a test of the apparatus and method used, the preliminary experiments upon adults show some important characteristics of the rate of voluntary motion.

1. The rate of voluntary motion in a given joint of a given individual is very constant.

The following tables taken at random from many, show the degree of variation in individual successive measurements.

(Explanation of tables: I., outer joint of the forefinger; II., middle joint of forefinger; III., metacarpo-phalangeal joint of forefinger; IV., wrist; V., elbow; VI., shoulder; VII., free tap. Each number in the tables shows the number of taps made in a period of five seconds.)

Table I. Subject, L. B. February 18, 1892.

	I	II	III	IV	V	VI	VII
Right.	27	28	35	37	34	33	34
	26	27	35	34	33	29	38
	28	30	35	34	33	29	38
	28	27	33	38	34	29	35
	32	30	36	35	33	34	34
Left.	27	26	31	34	31	25	38
	31	28	30	36	32	25	31
	27	26	29	35	34	25	35
	26	28	30	33	36	26	32
	28	28	30	34	34	25	33

SUBJECT, E. C. S. FEBRUARY 27.

	I	II	III	IV	V	VI	VII
Right.	19 17 26 23 22	30 29 22 25 29	30 30 28 30 30	39 37 37 36 34	35 32 27 37 30	32 24 29 25 27	38 43 41 39 37
Left.	16 17 17 18 16	18 16 17 15 18	26 25 23 26 23	27 25 25 27 24	24 29 21 22 23	22 18 21 17 18	26 31 27 30 27

SUBJECT, W. B. FEBRUARY 29.

	I	II	III	IV	V	VI	VII
Right.	23 23 23 22 22	23 22 23 23 21	25 24 23 27 24	29 27 23 30 27	39 41 40 37 39	28 30 20 28 28	36 38 36 40 39
Left.	17 15 15 11 11	15 16 18 18 17	20 20 20 22 23	20 21 21 24 23	24 23 27 25 25	17 22 21 17 23	27 26 24 24 26

Mean Variation of Individual Results from the Mean.

In the case of 239 mean rates, each obtained from five single tests on W. B., the mean value of the mean individual variations $\left(\frac{\Sigma v.}{n}\right)$ is .85 taps in five seconds; and two thirds of the $\left(\frac{\Sigma v.}{n}\right)$ values are less than 1.1 taps in five seconds. In 82 such mean rates obtained from L. B., the mean value of $\left(\frac{\Sigma v.}{n}\right)$ is 1.09 taps in five seconds and two thirds of these $\left(\frac{\Sigma v.}{n}\right)$ values are less than 1.4 taps in five seconds. Of 355 mean rates obtained from three subjects, 96% show $\left(\frac{\Sigma v.}{n}\right)$ values less than two taps in five seconds. So far as these experiments have

weight accordingly, the probability is .96 that two records of the maximum rate of voluntary movement, taken as nearly as possible under the same outer and inner conditions, will differ less than two taps in five seconds. It is altogether likely that there are individuals in whom the variability would be somewhat greater.[1]

2. The rate of voluntary movement undergoes slight and gradual but measurable changes due to changes in the subject.

Effects of Local Cold. The application of snow to the left forefinger resulted in reductions of the rates of the joints of that finger, amounting to 1.6, 1.6 and 1.4 taps in 5″; but caused no corresponding change in the rates of the other joints.

Effect of Local Fatigue. Fatigue was induced by rapid and continuous voluntary movement of the joint, in the same manner as that required in tapping with that joint. Sometimes fatigue was hastened by weighting the joint. Tests were taken from time to time after ten minutes' work. The final tests were taken after one to three hours' work. In one case (Table III.), fatigue was induced in the left hand by gripping upon the Galton dynamometer. In all cases the process becomes excessively painful. Following tables give the results gained. Explanation of tables : The joints are indicated by the Roman numerals from I. to VI., beginning with the outer finger joint. VII. indicates the free tap. R=Right side. L=Left side. The exponent a means that the series following was taken before fatigue. The exponent w indicates that every single record represented in the series of averages following was taken while one of the joints upon that side was in state of extreme fatigue. The exponent b indicates that every single record represented in the series of averages following was taken while one of the joints upon the opposite side was in a state of extreme fatigue. The wearied joint and the corresponding joint on the other side are indicated by underscoring their records. The records of other joints upon both sides are given to show the general motor ability before and during the local fatigue. For convenience of reference, each set of results has been numbered. In (33) a was taken at the point

[1]The constancy of the maximum rate of motion is indicated by the small limits within which the racing records of a given individual vary. Notwithstanding the very large number of motions made by a horse in running one mile, a dozen successive race-records are not expected to have a gross variation of more than two or three seconds, if the horse, the track, the weather, etc., are each time in about the same condition. The same holds true of bicycle riders, oarsmen etc.

of extreme fatigue, b after an interval of recovery. In (36 and 38) a, b, c, and d were taken in order at various periods from thirty minutes to two and one-half hours. In (42) a, b and c were taken at periods of fifteen, forty-five and one hundred and fifty minutes, work being continued all the time.

TABLE II.

RATE OF TAPPING. I. P. 1–6. RECORD W. B.

	I	II	III	IV	V	VI	VII	No.
Feb. 2, R^a	22.6	22.4	24.6	29.6	39.2	28.6	38.	1
	.4	.7	1.1	1.8	1.1	.6	1.4	
" 2, R^b	20.2	19.8	23.2	28.8	35.4	26.2	38.8	2
	.3	.7	.2	1.	1.6	1.7	.6	
" 2, L^w	13.4	16.6	21.	21.4	24.8	20.6	25.4	3
	1.9	.9	.6	.9	.1	1.6	1.9	
" 5, R^a	19.2	22.2	29.6	33.	37.2	31.	40.	4
	.7	.6	.9	1.1	.3	2.8	1.6	
" 5, R^b	18.4	19.6	22.8	28.4	34 8	30.2	38.2	5
	.6	.5	1.0	1.2	.9	1.8	1.	
" 5, L^w	18.4	18.2	18.8	21.4	27.4	23.8	25.8	6
	.6	.7	1.	1.1	.9	1.4	1.	
" 8, R^a	21.	22.6	26.6	31.4	35.8	28.8	38.	7
	.4	1.1	.5	1.9	1.4	1.4	.8	
" 8, R^b	21.	20.6	26.	28.4	34.	30.2	36.2	8
	.8	1.7	.4	2.	.2	1.	.6	
" 8, L^w	19.4	18.6	22.	23.8	27.2	27.4	28.2	9
A.M.	1.	.6	.8	.6	1.5	1.1	1.4	
" 9, R^a	19.8	19.2	25.6	31.8	38.2	28.4	38.6	10
	.3	1.7	1.2	1.7	2.	.9	1.1	
" 9, R^b	20.2	19.6	27.2	30.8	30.2	29.4	33.	11
	.5	.9	.7	.7	.2	1.1	2.4	
" 9, L^w	18.6	19.	22.	25.8	23.2	25.2	25.6	12
P.M.	.9	.4	1.2	1.1	1.	1.	.9	
" 9, R^a	20.8	21.8	26.8	29.4	34.8	29.2	36.8	13
	1.	.6	1.4	1.0	.6	1.	1.1	
" 9, R^b	21.3	21.	26.6	29.6	31.8	25.8	36.2	14
	.2	1.2	.7	.5	1.4	1.4	.6	
" 9, L^w	18.	20.	22.4	25.	25.2	20.6	25.8	15
A.M.	.4	.8	.8	1.2	.3	1.1	1.0	
" 11, L^a				24.8	26.6	21.4	27.4	16
				.8	.5	1.4	.9	
" 11, L^b				23.0	24.1	21.6	25.8	17
				.8	.2	1.1	.6	
" 11, R^a				33.	35.6	27.6	37.4	18
				.8	1.9	1.1	1.7	
" 11, R^w				30.6	28.8	26.4	30.2	19
P.M.				.6	1.7	.5	2.1	
" 11, L^a			23.6	25.2				20
			.5	.3				
" 11, L^b			21.3	23.9				21
			.6	.5				
" 11, R^w			21.0	31.				22
			.5	.1				

TABLE II.—*Continued.*

RATE OF TAPPING. RECORD W. B.

	F. Finger.			M. Finger.			R. Finger.			L. Finger.			Thumb.			
Feb. 10	I	II	III	I	II	III	I	II	III	I	II	III	I	II	III	
Rᵃ	22.6	22.6	25.8	21.6	23.2	26.4	20.2	20.6	24.8	20.	20.4	24.8	20.6	21.	23.8	23
	1.1	1.1	.6	1.3	.3	.9	1.1	1.3	.3	1.2	1.1	.6	1.6	.8	.6	
Lᵃ	18.8	20.4	22.4	13.2	17.8	22.	12.4	17.2	19.6	13.4	16.2	18.	17.8	16.4	18.6	24
	.6	.3	.5	.6	.6	.4	.7	.3	.9	.9	.1	.8	.6	.5	1.3	
Lᵇ	17.4	18.4	19.2			21.6			20.2			18.6			20.	25
	.5	.1	1.			.6			.3			.9			1.2	
Rʷ	22.2	21.6	24.6			26.			24.2			22.6				26
	.7	1.5	.8			.8			.7			.7				

RATE OF TAPPING. RECORD W. B.

Feb. 17	I	II	III	IV	V	VI	VII		
Rᵃ	21.2	21.2	25.8	32.6	37.8	27.4	38.		27
	.7	.6	.3	1.4	.7	1.4	1.2		
Lᵃ	18.	18.8	23.4	24.2	27.	22.	26.6		28
	.5	.6	.5	1.9	.4	.4	.7		
Rʷ			25.8	32.6	32.2	26.4	36.8		29
			1.	3.6	1.4	1.3	2.5		
Lᵇ			22.4	23.	25.	23.	26.		30
			1.1	.8	.8	.4	.4		

RATE OF TAPPING. L. B. RECORD.

Feb. 17	I	II	III		IV	V	VI		
Left A.	23.2	25.6	30.8		31.6	31.6	25.2		31
	1.4	1.7	1.9		3.	.8	.7		
Rᵃ	24.6	26.1	34.3		36.	35.3	31.6		32
	.6	1.6	.4		.2	1.0	.7	A's taken first.	
	B	B			B	A	*B*	B's after an in-	
Rᵇ	27.2	28.	29.4	31.6	34.8	34.8	30.	terval of recov-	33
	1.4	.8	1.2	1.3	1.	.7	1.2	ery.	
	B	*B*	A	*B*	*B*	*B*	*B*		
Lʷ	26.	27.1	25.7	29.8	33.4	32.2	26.2		34
	.4	1.4	.3	1.3	1.4	2.1	1.6		

TABLE II.—*Continued.*

RATE OF TAPPING. RECORD F. B. D.

Feb. 18 7 P. M.	I	II	III	IV	V	VI	VII	
R^a	21.6	30.	38.1	55.2	44.	26.	61.8	35
	.4	1.3	.5	4.1	3.8	1.2	.6	
R^b	20.	33.3	(a) 54.	50.	49.	26.	58.5	36
	0.	.3	5.3	0.	1.	0.	1.	
			(b) 52.3					
			3.9					
			(c) 50.6					
			3.					
			(d) 50.6					
			.3					
L^a	19.	24.2	37.6	40.6	38.	24.	41.	37
	0.	.7	1.	1.3	2.5	.6	2.	
L^b	20.6	25.	(a) 29.3	41.	34.	25.	42.	38
	.8	1.	2.4	1.	.6	0.	2.	
			(b) 30.2					
			5.2					
			(c) 30.7					
			.7					
			(d) 34.					
			1.7					

RATE OF TAPPING. RECORD E. C. S.

Feb. 27	I	II	III	IV	V	VI	VII	
R^a	21.	27.	30.7	36.4	32.8	27.4	39.6	39
	2.6	2.8	2.2	1.2	1.7	2.5	1.9	
L^a	16.8	16.8	24.8	25.6	23.8	19.2	28.2	40
	.6	1.	1.3	1.1	2.1	1.8	1.8	
R^b	23.4	25.4	27.3	37.4	45.*	26.	40.8	41
	2.1	1.8	1.2	1.3	6.4	2.2	1.4	
L^b	16.2	18.6	(15 m.) 20.8	26.8	24.2	21.6	29.4	42
	.3	.6	.6	1.	1.8	2.	1.9	
			(45 m.) 19.8					
			.6					
			(2½hrs.) 18.4					
			1.2					

*The individual rates were 54, 39, 39, 50, 38. The 54,50 rates were visibly the shiver of "voluntary tetamus."

TABLE III.

RATE OF TAPPING. WEARYING BY GRIP OF LEFT. W. B.

	I	II	III	IV	V	VI	
R^a	21.	23.	25.	28.	34.	32.	43
R^b	21.	24.	25.	28.	38.	30.	44

1. Fatigue begins to show itself by a perceptible lowering of rate after ten or fifteen seconds' work.

This fact appeared so clearly that it proved necessary to allow a brief interval of rest between each five-second period of work, and longer intervals every second or third time.

(NOTE—See in Dresslar's Influences affecting Rate, the evidence of fatigue in the course of 300 taps.)

2. After ten to fifteen minutes' work the reduction in rate is considerable. Thereafter the reduction goes on more slowly. By three hours' work E. C. S. reduced his left wrist to momentary helplessness.

3. Partial recovery takes place very quickly.

This fact came out with such certainty that it proved necessary to keep the wearied joint hard at work throughout the series except during the 5″ intervals when a test was in progress.

4. Complete recovery from excessive fatigue takes place slowly.

Compare Nos. 10-12 with 13-15, Table II., for evidence of the persistence of fatigue during 2-4 hours' rest.

5. Working a joint induces a local fatigue, which does not perceptibly affect the rate of remote joints. In some cases the fatigued joint's rate is reduced, while nearly all the other joints show an increase of rate.

6. Taking the average yearly increase of rate as a standard, the change of rate induced by fatigue is very great.

It will be shown later on, that between the ages 6 and 16 the average yearly increase of rate ranges from .74 to 1.44 taps in five seconds, and that a yearly gain of two taps or over in five seconds has a probability of .17 or .33 less than an even chance. The decrease of rate by local fatigue induced by 1 to 2½ hours' hard work is shown to be as follows:

W. B., 5, 4.6, 6.8, 1.2, 5.6; L. B., 5.1; F. B. D., 8.3; E. C. S., 5.

As noted above, by 3 hours' work, E. C. S. reduced the rate of the left wrist practically to zero.

In all but one instance these amounts are equivalent to the growth of three or four years.

7. I am not prepared to say positively whether local fatigue in a joint affects the rate in the corresponding joint on the other side. The facts are as follows: In my own case (as shown in the W. B. records) in every trial (left shoulder, elbow, wrist, hand-finger joint and middle finger joint; and with the right elbow, wrist and hand-finger joint), extreme local fatigue in a joint was followed by a diminution in the rate of the corresponding joint on the other side, while other joints, upon both sides, showed no corresponding change in general motor ability.

As results in which the experimenter is the subject, are always justly subject to doubt because of the possible unconscious influence of expectation, I observe,

(a) In Table III., where it is shown that wearying the left hand by gripping was followed by no discernible change in rate of any right side joint, the contrary result was fully expected.

(b) In my case, the most rapid joint is the elbow. The rate of the free tap is, therefore, mostly determined by the rate of the elbow. It will be seen in each case that the rate of elbow and free tap closely agree. If, now, those cases in which either elbow was wearied be examined, it will be seen not only that the rate of the other elbow is lowered, but also that the rate of the free tap on the other side was lowered. This result was not anticipated, and only came to notice when calculations and comparisons were made subsequent to the conclusion of the experiments.

(c) On February 9th, A. M. (Table II., 10-12), the left elbow was wearied, the experiment concluding about noon. At about 2 P. M. of the same day work was resumed, the left shoulder being wearied. The record for February 9th, P. M. (Table II., 13-15), shows that neither the left elbow fatigued in the morning nor the right elbow have recovered their normal rate, although the rate of other joints proves that there was no general decline of motor ability. This result was not anticipated and was not discovered until later.

The results from L. B. confirm the results from W. B.

The results from F. B. D. show an extraordinary increase in the rate of the joint corresponding to the wearied joint, in the presence of no considerable change in the rate of other joints on either side. This wholly unexpected result was thoroughly verified.

The results given from E. C. S. appear to confirm the result from W. B. In a second very thorough trial, the left wrist being fatigued to the point of temporary helplessness,

no bilateral effect appeared. I have had the misfortune to lose this valuable negative record. I have been strongly inclined on this account to withhold all results on this point from publication. I have concluded to publish the results, specially emphasizing the entirely negative result from E. C. S. and withholding, until further investigation, any positive conclusions as to the question involved.

The Effect of Amplitude of Movement on Rate. I have already referred to the conclusion of von Kries (p. 138), that "the extent of the movement has small influence upon its duration," motions of a certain medium extent appearing to be carried out more quickly than longer or shorter ones. With this result my observations are in entire agreement. The following table gives the number of double excursions of the several amplitudes indicated in five seconds:

TABLE IV.

Extent of Excursion in mm.	1	5	10	15	20	25	30	40
Number of double excursions in 5 sec.	27	26	27	30	30	30	25	20
	28	29	28	30	29	30	26	23
	24	28	28	29	30	30	24	24
	24	29	30	28	30	29		26
	27	32	29	31	31	28		
	27	29						
	25							
Average.	26.	28.8	28.4	29.6	30	29.4	25	23.2

Maximum Rate of Tapping by Adults. A sufficient number of adults have not been tested to permit the establishment of a normal mean rate or to show the individual variation. For the results with unwearied joints see Table II. Nos. 1, 4, 7, 10, 13, 16, 18, 20, 23, 24, 27, 28, 31, 32, 35, 37, 39, 40 and 43.

SCHOOL EXPERIMENTS ON RATE.

(NOTE—I am indebted to Superintendent Marble and the school authorities of Worcester, and to Dr. Franz Boas, for the opportunity of making these tests.)

Seven hundred and eighty-nine school children of the City of Worcester, Mass., ranging in age from 5 to 16 years, were tested with the apparatus and by the method described. The shoulder, elbow, wrist and metacarpo-phalangeal joint of the forefinger on each side were so tested.

Classification of Results.

Forty-six individuals (twenty-six boys and twenty girls) were more or less left handed. Their records are taken account of separately (175). Twenty individuals (fourteen boys and six girls) of five years of age were tested. The boys' record is printed in Table VI. The principal calculations and results of this paper refer to the 723 right-handed individuals ranging in age from 6 to 16. The 5,784 single results obtained from these individuals were classified according to the age and sex of the individual, and according to the side and joint used.

Method of Treating Results.

The several results belonging to each class,—for example, to the right finger of the boys of six,—were tabulated so as to show the different rates found in that class (Column I.), (Table V. page 152) and the number of times each of these rates occurs (Column II.). Each rate found was multiplied by the number of times it occurs; and the sum of these products (Column III.) was divided by the sum of individual cases. The differences between this result, the arithmetical mean, and each rate found were taken (Column IV.), and these residuals were squared (Column V.). Each squared residual was multiplied by the number of times the corresponding rate occurs. The sum of these weighted squared residuals (Column VI.) was divided by the number of individual cases. The square root of this quotient is the mean individual variation, showing the limits of distance from the mean, within which 68.3% of the individual results fall. This individual mean variation divided by the square root of the number of cases, gives the mean variation of the mean. The probability is .683 that the true mean lies within the limits thus determined.[1]

The tabulation and calculation for boys' right finger, 6, are given as an example. A designates the arithmetical mean; μ the mean individual variation and μ_0 the mean variation of the mean. The values of A, μ, and μ_0 for each class, are given in Table VI. page 152, andare represented graphically in chart I. In the latter the values of A are connected by

[1] The so-called probable errors, i. e., the errors whose probability is .50, may be found by multiplying the values of μ and μ_0 by .6745.

solid lines, and the values of μ and μ_o by dotted lines on each side of the solid line.[1]

TABLE V.

RATE BOYS 6. RIGHT FINGER. n=26.

Rates found = R.	No. of cases at each rate = n.	R x n.	Residuals. = v.	v^2	$n v.^2$
I	II	III	IV	V	VI
15	1	15	4.5	20.25	20.25
16	1	16	3.5	12.25	12.25
17	1	17	2.5	6.25	6.25
18	4	72	1.5	2.25	9.00
19	3	57	.5	.25	.75
20	10	200	.5	.25	2.50
21	3	63	1.5	2.25	6.75
22	2	44	2.5	6.25	12.50
23	1	23	3.5	12.25	12.25
	26	507			82.50 $=\Sigma\ v.^2$

507÷26=19.5=A.

$$\sqrt{\frac{82.50}{26}}=1.78+=\mu_R$$

$$\frac{1.78}{\sqrt{26}}=.34+=\mu_{OR}.$$

[1] It must be kept in mind that the mean rates do not represent quantities which exist in nature. Each mean is a function of the individual rates found in a given class, and only in connection with the mean individual variation and the mean variation of the mean itself, does it represent the probable rate and distribution of rates for that class.

TABLE VI.

RATE: AGE 5.

		Boys, n=14.			Girls, n=6.
		A	μ	μ_o	Omitted, because so few.
R	F	19.6	2.82	.8	
	W	20.1	3.66	1.0	
	E	22.7	2.90	.8	
	S	18.4	2.72	.7	
L	F	17.3	3.34	.9	
	W	17.	3.39	.9	
	E	18.2	3.23	.9	
	S	17.	2.38	.6	

RATE: AGE 6.

		Boys, n=26.			Girls, n=28.		
		A	μ	μ_o	A	μ	μ_o
R	F	19.5	1.78	.3	19.8	2.56	.5
	W	23.	2.72	.5	21.6	2.57	.4
	E	23.5	1.45	.3	22.7	2.33	.4
	S	19.8	2.81	.6	19.9	2.71	.5
L	F	18.	1.56	.3	18.	2.76	.5
	W	19.7	2.32	.5	18.9	2.33	.4
	E	20.4	2.47	.5	19.7	2.06	.4
	S	18.2	2.34	.5	17.9	2.43	.5

RATE: AGE 7.

		Boys, n=35.			Girls, n=32.		
		A	μ	μ_o	A	μ	μ_o
R	F	21.	2.64	.4	20.7	2.46	.4
	W	23.7	2.91	.5	23.1	2.7	.5
	E	24.2	3.71	.6	23.2	1.95	.3
	S	20.5	2.48	.4	20.2	2.66	.5
L	F	19.1	2.4	.4	19.1	3.23	.6
	W	20.2	2.6	.4	20.	2.46	.4
	E	20.9	3.07	.5	21.5	2.96	.5
	S	18.8	2.5	.4	18.8	2.67	.5

TABLE VI.—*Continued.*

RATE: AGE 8.

		Boys, n=33.			Girls, n=32.		
		A	μ	μ_o	A	μ	μ_o
R	F	23.1	2.56	.4	22.2	2.74	.5
	W	26.3	2.83	.5	24.3	2.73	.5
	E	26.1	2.57	.4	24.4	2.68	.5
	S	22.3	2.26	.4	21.9	3.08	.5
L	F	20.5	1.91	.3	19.7	2.71	.5
	W	22.2	2.88	.5	21.	2.69	.5
	E	22.3	2.61	.4	21.6	2.27	.4
	S	20.2	2.69	.5	20.2	2.64	.5

RATE: AGE 9.

		Boys, n=43.			Girls, n=36.		
		A	μ	μ_o	A	μ	μ_o
R	F	24.4	4.15	.6	24.	2.69	.4
	W	27.8	3.5	.5	25.5	3.26	.5
	E	28.2	3.52	.5	25.4	3.62	.6
	S	24.1	3.65	.6	22.7	3.26	.5
L	F	21.5	3.6	.5	20.6	2.92	.5
	W	23.6	3.84	.6	22.4	2.99	.5
	E	23.8	3.67	.6	22.6	2.91	.5
	S	20.9	3.57	.5	20.8	3.79	.6

RATE: AGE 10.

		Boys, n=37.			Girls, n=35.		
		A	μ	μ_o	A	μ	μ_o
R	F	25.2	2.29	.4	25.8	3.36	.6
	W	28.5	3.34	.5	28.5	3.66	.6
	E	28.1	3.60	.6	27.5	2.96	.5
	S	22.6	2.75	.4	22.6	2.76	.5
L	F	22.4	3.15	.5	22.3	2.64	.4
	W	24.5	3.70	.6	24.3	2.42	.4
	E	24.7	2.74	.4	24.	2.51	.4
	S	20.5	2.85	.5	21.6	2.62	.4

TABLE VI.—*Continued.*

RATE: AGE 11.

		Boys, n=36.			Girls, n=35.		
		A	μ	μ_o	A	μ	μ_o
R	F	27.	3.60	.6	27.1	3.44	.6
	W	30.3	4.62	.8	30.4	4.30	.7
	E	29.3	3.41	.6	28.6	3.44	.6
	S	24.1	2.93	.5	24.9	3.30	.6
L	F	23.9	2.57	.4	24.9	3.48	.6
	W	25.9	3.69	.6	26.3	4.02	.7
	E	26.	3.41	.6	26.2	3.57	.6
	S	21.5	2.57	.4	23.6	3.44	.6

RATE: AGE 12.

		Boys, n=33.			Girls, n=34.		
		A	μ	μ_o	A	μ	μ_o
R	F	29.3	5.4	.9	28.2	3.98	.7
	W	31.6	5.27	.9	31.6	5.18	1.0
	E	29.9	4.12	.7	29.4	4.90	.8
	S	25.	3.32	.6	25.7	4.35	.7
L	F	26.3	3.84	.7	25.8	3.44	.6
	W	26.9	3.70	.6	27.	4.90	.8
	E	26.3	3.76	.6	26.2	4.56	.8
	S	22.4	3.59	.6	23.6	3.64	.6

RATE: AGE 13.

		Boys, n=34.			Girls, n=34.		
		A	μ	μ_o	A	μ	μ_o
R	F	28.7	3.49	.6	30.3	4.52	.8
	W	32.3	3.69	.6	33.2	6.03	1.
	E	31.	4.44	.8	30.5	6.20	1.1
	S	25.5	4.52	.8	27.5	4.32	.7
L	F	26.1	3.1	.5	26.7	4.85	.8
	W	27.6	3.72	.6	28.6	4.89	.8
	E	27.5	3.7	.6	28.6	4.15	.7
	S	23.7	3.81	.7	25.2	3.83	.7

TABLE VI.—*Continued.*

RATE: AGE 14.

		Boys, n=41.			Girls, n=33.		
		A	μ	μ_o	A	μ	μ_o
R	F	31.5	3.69	.6	29.5	3.02	.5
	W	33.	3.8	.6	30.3	3.79	.6
	E	32.7	4.63	.7	28.8	2.83	.5
	S	27.2	3.49	.5	26.6	3.17	.5
L	F	[1]27.5	3.69	.6	26.8	2.89	.5
	W	29.	2.61	.4	28.	3.70	.6
	E	28.6	3.62	.6	26.9	3.48	.6
	S	24.3	3.49	.5	23.8	3.34	.6

[1]n=40 in L, F.

RATE: AGE 15.

		Boys, n=32.			Girls, n=31.		
		A	μ	μ_o	A	μ	μ_o
R	F	31.6	4.02	.7	29.1	3.44	.6
	W	34.2	4.46	.8	30.9	4.31	.8
	E	31.5	3.72	.7	29.3	3.84	.7
	S	26.3	3.29	.6	26.	3.89	.7
L	F	28.3	3.46	.6	26.7	3.23	.6
	W	29.5	3.60	.6	28.3	3.97	.7
	E	28.7	3.61	.6	27.6	4.01	.7
	S	24.7	3.58	.6	25.4	4.14	.7

RATE: AGE 16.

		Boys, n=26.			Girls, n=17.		
		A	μ	μ_o	A	μ	μ_o
R	F	33.9	4.92	1.0	31.3	4.66	1.1
	W	35.9	5.16	1.01	30.1	4.93	1.09
	E	32.7	3.90	.8	33.3	4.52	1.2
	S	28.7	3.42	.7	27.9	3.44	.8
L	F	30.7	5.08	1.0	28.6	4.01	1.0
	W	33.1	4.70	.9	29.5	3.87	.9
	E	30.7	2.68	.5	28.2	3.75	.9
	S	26.6	3.52	.7	26.2	3.55	.8

The Degree of Trustworthiness of Results.

I have endeavored to state fully the conditions under which the foregoing tests were made, so that the *a priori* probability of their trustworthiness might be estimated. There are, however, two ways of determining from the results themselves their probable degree of trustworthiness. From whatever known or unknown sources of error the work may have suffered, the net errors are probably within the limits thus ascertained.

1. The mean errors of the means.

Inspection of the tables or charts shows that the limits of mean errors of the means are in all cases absolutely small, and small in comparison with the values of the corresponding means. Further examination shows:

Average of the 160 values of μ_o for ages 6-15,	.57 taps in 5″
Sixty-eight per cent. of these values range from 0 to	.67 " " "
Maximum value of μ_o,	1.1 " " "

Seventy-seven per cent. of the values of μ_o are less than 2% of their corresponding means.

In sum, if an equal number of individuals, corresponding in age and sex to any class for whom means are here given, be tested under the same conditions, the probability is .683 that the resulting mean will not differ from that here given by more than .67 taps in 5″, and the probability is .978 that the resulting mean will not differ from that here given by more than one tap in 5″.

The relatively small number of individuals of 16 upon whom measurements could be obtained renders the means at that age somewhat less reliable:

Average value of μ_o for age 16,	.9 taps in 5″
Maximum value of μ_o,	1.2 " " "
Average $\frac{\mu_o \cdot 100}{A}$	3% " " "

2. An independent means of judging the degree of trustworthiness of the means is furnished by a comparison of the results from boys with those from girls. It cannot be assumed, and is in fact later shown to be untrue, that the rates for boys and for girls of the same age are approximately equal. On the contrary there appear clearly defined differences varying with the age examined. But if, the fact of characteristic differences between the two sexes having been established, it should appear that the two sets of wholly independent means differ from each other within very narrow limits, then it is probable that two sets of independent means obtained from individuals of the same sex would differ from each other within still narrower limits, certainly not wider ones. If the values of (B-G) for the 88 possible comparisons

(11 ages, 8 joints) between corresponding mean rates of boys and girls be determined, the differences in the mean rates of boys and girls is shown to be very small. Of the 88 values of (B-G),

3	differ	by	3	taps	or	over	in	5	seconds.
14	"	"	2	"	"	"	"	"	"
19	"	"	1	"	"	"	"	"	"
52	"	"	less than 1 tap				"	"	"

In 59 of 88 cases, (B-G) $<$ (μ_0 B+μ_0G)[1]

That is, in 67% of the cases the difference between the mean rate for boys and the corresponding rate for girls is within the limits of the mean errors of the means compared.

Mean Variation: It will be noted that the mean individual variation is subject to considerable fluctuation. Examination shows that periods of most rapid acceleration are generally periods of widening individual variation, while periods of greatest retardation or decline are periods of narrowing individual variation. The mean individual variation rarely (seven times out of 184) exceeds one tap per second.

Extreme Limits of Variation of Rate.

The lowest mean rate found is 17.9 taps in five seconds (girls left shoulder, 6), the highest mean rate found is 35.9 taps in five seconds (boys, right wrist, 16). If the corresponding mean variation be taken into account, we have 15.5 and 41.2 as the limits within which nearly all individual cases may be expected to fall. Not all, however. In order to show the fact and the degree of probability of extreme high and low rates, I have tabulated the individual results which show rates of less than 4 and of more than 8 per second.[2]

Representing by A the mean rate of a given age; by A$\pm\mu$ the mean with its individual variation; and by N the limits within which 99% of all individual cases fall, we have:

---

[1] μ_0B=mean var. of a mean rate of boys. μ_0G=same for a mean rate of girls.

[2] Very high rates were usually repeated. The best single record was made by a girl, F. W., of 12 in the sixth grade, as follows: R. F., 40; W., 48. (Second trial 47), E., 40; S., 34; L. F., 32; W., 43; E., 40; S., 34.

She looked the type of robust health. When asked if she played the piano she said, "Only by ear; but I play base ball though," adding a moment later, "I can strike two over an octave on the piano." Another of many interesting individual records is that of A. C., a girl of 13, who has taken lessons upon the violin for two years: R. F., 42; W., 43; E., 40; S., 34; L. F., 42; W., 42; E., 34; S., 26.

The high rates of the joints most involved in playing the violin in connection with the low rate of the left shoulder form an interesting picture of the effects of special practice.

	A	A+μ	N
Upper Limit	35.9	41.2	45 taps in 5″
	A	A—μ	N
Lower Limit	17.9	15.5	12 " " "

Of the 5,944 single measurements (including those from five year old children) we have,

Below a rate of 3 per second,	2	cases.
From 3 to 4 per second,	166	"
" 4 " 8 " "	5,709	"
" 8 " 9 " "	61	"
9 per second or over,	6	" [1]

Variation of Rate with Age.

Inspection of the tables or charts shows for both sexes and for all joints an increase of rate with age. The total amount of increase in the rate of each joint in the ten years from 6 to 16 is shown in the following table. A change in decimal point gives the average yearly increase.

Total increase in rate between ages 6 and 16, in terms of x taps in 5″:

Boys' right—F., 14.4 ; W., 12.9 ; E., 9.2 ; S., 8.9.
Boys' left — F., 12.7 ; W., 13.3 ; E., 10.3 ; S., 8.4.
Girls' right—F., 11.5 ; W., 8.5* ; E., 7.4* ; S., 8.0.
Girls' left—F., 10.6 ; W., 10.6 ; E., 8.5* ; S., 8.3.

*Higher at 13 than at 16.

The amount of increase is not, however, the same each year. Table VII. gives the amount of increase or decrease for each joint in each year period. Numbers less than the average increase are printed in italics. Table VIIa. shows the periods of most obvious acceleration and retardation of growth.[2]

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[1]For the behavior of the different joints in successive years, see p. 66.
[2]This table is given not to show the distribution, but the extreme limits of rates found.

Table VII.

Showing Amount of Increase or Decrease in the Rate of Each Joint Each Year from 6–16 in Terms of X Taps in 5.″

Ages		6-7	7-8	8-9	9-10	10-11	11-12	12-13	13-14	14-15	15-16
Boys' Right.	F	1.5	2.1	1.3	.8	1.8	2.3	–.6	2.8	.1	2.3
	W	.7	2.6	1.5	.7	1.8	1.3	.7	1.5	.4	1.7
	E	.7	1.9	2.1	–.1	1.2	.6	1.1	1.7	–1.2	1.2
	S	.7	1.8	1.8	–1.5	1.5	.9	.5	1.8	.9	2.5
Boys' Left.	F	1.1	1.4	1.0	.9	1.5	2.3	–.2	1.4	.8	2.4
	W	.5	2.0	1.4	.9	1.4	1.0	.7	1.4	.5	3.6
	E	.5	1.4	1.5	.9	1.3	.3	1.2	1.1	.1	2.0
	S	.6	1.4	.7	.4	1.0	.9	1.3	.6	.4	2.1
Girls' Right.	F	.9	1.5	1.8	1.8	1.3	1.1	2.1	–.8	–.4	2.1
	W	1.5	1.2	1.2	3.0	1.9	1.2	1.6	–2.9	.6	–.8
	E	.5	1.2	1.0	2.1	1.1	.8	.9	–1.7	.5	.8
	S	.3	1.7	.8	–.1	2.3	.8	1.8	–.9	–.6	1.9
Girls' Left.	F	1.1	.6	.9	1.7	2.6	.9	.9	.1	–.1	1.9
	W	1.1	1.0	1.4	1.9	2.0	.7	1.6	–.6	.3	1.2
	E	1.8	.1	1.0	1.4	2.2	.0	2.4	–1.7	.7	.6
	S	.9	1.4	.6	.8	2.0	.0	1.6	–1.4	1.6	.8

Table VII a.

Every joint shows an increase greater than the average yearly increase for that joint:

In boys, at ages 7-8, 10-11, 15-16. In girls, 10-11.

Seven of the eight joints show accelerated growth :

In girls, 12-13.

Six of the eight joints show accelerated growth :

In boys, 13-14. In girls, 9-10.

Five of the eight joints show accelerated growth:

In boys, 8-9, 11-12. In girls, 6-7, 7-8, 8-9, 15-16.

Five of the eight joints show an increase of rate less than the average yearly increase for that joint, or a decrease :

In boys, 12-13. In girls, 11-12.

Seven of the eight joints show retardation, or decrease :

In boys, 6-7. In girls, 14-15.

Eight joints show retardation, or decrease :

In boys, 9-10, 14-15. In girls, 13-14.

The degree of acceleration or retardation of growth is shown graphically for each joint each year in the rate chart (I.) by the directions of the solid lines connecting the year-

averages. The plus or minus value of the angle which this line makes with the base line is measured by $\frac{\pm \Delta y}{\Delta t}$, Δt being the constant, 25 mm., and $\pm \Delta y$ being the increase or decrease for the year.

It is obvious from the tables and from the charts that there are certain periods when all or nearly all the joints grow at an accelerated rate, and other periods when all or nearly all grow at a retarded rate or even decline in ability; and it is clear what those periods are. Table VII a. The periods of most considerable and most significant decline are for boys from 14 to 15 and for girls from 13 to 14. It will be observed that in each of these cases the period of decline is preceded by a period of acceleration and is followed by a more or less rapid recovery.[1]

Comparison of Right and Left.

Method of Calculation.

The mean differences between the rates of right side joints and of the corresponding left side joints are obtained from the data in two arithmetically independent ways. (a) The mean rate for a given joint may be compared with that of the corresponding joint on the other side; or (b) the mean of the individual differences between corresponding joints may be ascertained. Both processes must give the same result and they furnish a check against error in calculation.

The values of (r-l) have a higher probability than the values of r or of l.[2] This would be expected a priori, if it were assumed that any degree of correlation exists between corresponding right and left joints. That is, in an entirely new set of individuals a variation in the mean of a right side joint would likely be accompanied by a variation in the mean of the corresponding left side joint, not of precisely the same amount but in the same direction.

That the values of (r-l) *are* more trustworthy than those of r or of l appears from the following consideration: On page 157 it was shown that the mean of the values of μ_0 is .56 taps in 5″ and that two thirds of the values of μ_0 are less than .67 taps in 5.″ If the value of (r-l) be found by the second (b) method described above, the mean variation of individual (r-l) values from the mean (r-l) and the mean variation of the mean (r-l) may be found by the same process as that described on page 151. These calculations were made.[3] The values of

[1]For a comparison of boys and girls in this respect see P. 174. For a discussion of the significance of the facts, see P. 200.

[2]r = mean rate of a right side joint; l = mean rate of a left side joint.

[3]See page 166, where an example of this calculation is given; and pages 167, the results for each pair of joints.

μ_{OB}[1] for ages 6 to 15 have an average value of .46 taps in 5″; and two thirds of these values are under .54 taps in 5.″

Mean Values of (r-l).

The mean rate of every right side joint is faster than that of the corresponding left side joint; and in every case the plus value of (r-l) is greater than its mean variation. In forty-seven of the 88 cases (11 ages, 4 joints, 2 sexes), (r-l) is greater than the corresponding mean individual variation. The values of each (r-l) may be found from the rate tables, page 153–156. The average values for each joint are as follows:

TABLE VIII.

	Mean of (r-l) values from 6 to 16.	Mean variation of yearly (r-l) values from mean $\frac{\Sigma v.}{n}$	
Boys' Finger	2.8	.68	
" Wrist	4.	.6	
" Elbow	3.4	.58	
" Shoulder	2.2	.53	All values in terms of X taps in 5.″
Girls' Finger	2.7	.62	
" Wrist	3.4	.8	
" Elbow	2.5	.58	
" Shoulder	1.7	.8	

Limits of Variation of (r-l).

TABLE IX.

2,992 individual values of (r-l) are distributed in the following proportions:

More than 15 taps in 5″		.001	
" " 10 and under, 15 taps in 5″		.012	
" " 5 " " 10 " " "		.132	
" " 0 " " 5 " " "		.642	.787 positive.
At	0	.111	
Below	0 and above—5 " " "	.100	.102 negative.
Under	—5 taps in 5″	.002	

Variations of (r-l) with Age.

1. The mean values of r and of l tend to vary together.

If the yearly increase or decrease in the mean rate of each right side joint be compared with the yearly increase or decrease in the mean rate of the corresponding left side joint, we have in all 80 comparisons (ten year periods, four pairs of

[1] μ_{OB} = mean variation of mean (r-l).

joints, boys and girls). In 91% of these 80 cases the r and l means both show increase or both show decrease. In 85% of the cases the r and l means both show acceleration or both show retardation.

2. The right side joints are subject to greater fluctuation of rate ability than are the left side joints.

Of 68 cases where r and l both show increase, r shows a greater increase than l in 46 cases. Of five cases in which both sides show decrease, r shows a greater decrease than l in four cases. Of the remaining seven cases, r shows a greater plus or minus variation than l in three cases. Of the whole 80 cases, 53 show a greater fluctuation in the right than in the left.

3. Variations in the values of (r-l) with age are dependent of course upon the relative rates of growth in r and l. The rate tables on pages 153–156 and the accompanying chart (Chart II.), exhibit these relations. I find myself only able to say on the whole that the right hand gains faster in certain years, but also loses more in other years, so that at 16 the difference between the two sides is almost the same as at the age of 6.

The total gains of r over l between ages 6 and 16, i. e., (r-l) at 16 minus (r-l) at 6, are as follows:

Boys: F 1.7; W –.4; E –1.1; S .5 taps in 5″ Average .17.
Girls: F .9; W 1.1; E –1.1; S –.3 " " " " .15.

Bilateral Symmetry and Asymmetry of Rate Ability.

It has been shown that the mean rate of right side joints is always greater than that of the corresponding left side joints. It is very desirable to know whether the two sides increase in ability together, and if not, to determine the degree of asymmetry in their development. This does not appear from a comparison of mean rates of right and left side joints in successive years. That widely varying degrees of bilateral correlation might exist in the presence of any given mean rates upon the two sides may be shown by the following illustration:

Let the individuals A, B, C, D and E have the rates 6, 9, 12, 8 and 5 in a right side joint, and the rates 4, 7, 10, 6 and 3 in the corresponding left side joint, it being undetermined which two rates belong to any individual. Assume three distributions of the rates among the individuals as follows:

		R	L	Difference		
I	A	6	4	2	Average R=8. Its	$\sqrt{\frac{\Sigma v^2}{n}}=\sqrt{6}$
	B	9	7	2	Average L=6. Its	$\sqrt{\frac{\Sigma v^2}{n}}=\sqrt{7.4}$
	C	12	10	2		
	D	8	6	2		
	E	5	3	2	Average D=2. Its	$\sqrt{\frac{\Sigma v^2}{n}}=0$
					Average R=8.	$\sqrt{\frac{\Sigma v^2}{n}}=\sqrt{6}$
II	A	6	3	3		
	B	9	10	–1		
	C	12	6	6	Average L=6.	$\sqrt{\frac{\Sigma v^2}{n}}=\sqrt{7.4}$
	D	8	7	1		
	E	5	4	1		
					Average D=2.	$\sqrt{\frac{\Sigma v^2}{n}}=\sqrt{5.7}$
					Average R=8.	$\sqrt{\frac{\Sigma v^2}{n}}=\sqrt{6}$
III	A	6	6	0		
	B	9	4	5		
	C	12	3	9	Average L=6.	$\sqrt{\frac{\Sigma v^2}{n}}=\sqrt{7.4}$
	D	8	7	1		
	E	5	10	–5		
					Average D=2.	$\sqrt{\frac{\Sigma v^2}{n}}=\sqrt{26.4}$

I. is a case of perfect bilateral correlation; II. a case of partial correlation; III. a case of non-correlation.

It is obvious that the value $\sqrt{\frac{\Sigma v^2}{n}}$ obtained from the column of differences between r and l is a measure of the fluctuation of individual (r-l) values from the mean (r-l). In order to determine whether a given value of $\sqrt{\frac{\Sigma v^2}{n}}$ indicates correlation or non-correlation, and what degree of either, it is necessary to determine what the value of $\sqrt{\frac{\Sigma v^2}{n}}$ would be if the relation between right side rates and left side rates *were determined by chance*, i. e., if the r values and l values were placed in separate boxes and paired, by drawing one from each box at a time.

This chance value of $\sqrt{\frac{\Sigma v^2}{n}}$ may be determined as follows:[1]

Let R=mean rate of a right side joint.
(R+r)=any individual rate by that joint.
μ_R=mean individual variation=$\sqrt{\frac{\Sigma v^2}{n}}$

[1] I am indebted for this method and development to Dr. Franz Boas, to whom I hereby return hearty thanks.

L=mean rate of corresponding left side joint.
(L+l)=any individual rate by that joint.
μ_L=corresponding mean individual variation.[1]

(1) Then $$P_{(R+r)}=\frac{1}{\mu_R\sqrt{2\pi}}\,\varepsilon-\frac{r^2}{2\mu_R^2}$$

and $$P_{(L+l)}=\frac{1}{\mu_L\sqrt{2\pi}}\,\varepsilon-\frac{l^2}{2\mu_L^2}$$

are the respective probabilities of any rates (R+r), and (L+l).

If these probabilities are independent of each other, the probability that (R+r) and (L+l) will occur together is:

(2) $$P_{(R+r)}\,P_{(L+l)}=\frac{1}{\mu_R\,\mu_L\,2\pi}\,\varepsilon-\frac{r^2}{2\mu_R^2}-\frac{l^2}{2\mu_L^2}$$

(R+r) — (L+l) = (R—L) + (r—l). Substituting u for (r—l).

(3) $$P_{R-L+u}=\int_0^{\infty}\frac{1}{\mu_R\,\mu_L^2\,2\pi}\,\varepsilon-\frac{r^2}{2\mu_R^2}-\frac{(r-u)^2}{2\mu_L^2}$$
$$=\frac{1}{\sqrt{2\pi}\,\sqrt{\mu_R^2+\mu_L^2}}\,\varepsilon-\frac{u^2}{2(\mu_R^2+\mu_L^2)}$$

It thus appears that if the individual rate (R+r) and the individual rate (L+l) are independent of each other, the mean individual variation in the values of (R+r)—(L+l) will be $\sqrt{\mu_R^2+\mu_L^2}$ that is, the square root of the sum of the squares of the mean individual variations of (R+r) and (L+l) respectively. The Table XI., page 167, and the Chart III., give the values of μ_0 and of $\sqrt{\mu_R^2+\mu_L^2}$ and of $\frac{\mu_0.100}{\sqrt{\mu_R^2+\mu_L^2}}$ for each pair of joints at each age from 6 to 16.

The last quantity shows the relation in per cent between the chance value and the actual degree of the bilateral asymmetry. The Table *X*., page 166, gives an example of the method of determining μ_R (for boys' finger, 6).

Table showing the method of calculating the mean variation of individual values of (r—l), about the average value of (r—l) for boy's finger, 6. Unit=1 tap in 5 seconds.

[1] For the purposes of this calculation R is used instead of r for the mean rate of a right side joint, and likewise L for l.

TABLE X.

Differences between r and l in individual cases=d.	No. of cases of each difference found=n.	dn.	Residuals =v.	v^2	nv^2
–2.	2	–4.	3.3	10.80	21.78
–1.	2	–2.	2.3	5.29	10.58
0.	4	0.	1.3	1.69	6.76
1.	5	5.	.3	.09	.45
2.	8	16.	.7	.49	3.92
3.	2	6.	1.7	2.89	5.78
4.	2	8.	2.7	7.29	14.58
5.	1	5.	3.7	13.69	13.69
	n=26	34.			$\Sigma v^2 = 77.54$

$$\frac{34}{26}=1.3+=\text{Av. (r—l)}.$$

$$\sqrt{\frac{77.54}{26}}=1.73-=\mu\ \text{(r—l)}.$$

$$\frac{1.73}{\sqrt{26}}=.3+=\mu_0\ \text{(r—l)}.$$

Table XI.

Rate: Bilateral Symmetry.

Age		Boys. [1] $\sqrt{\mu_B^2+\mu_L^2}$	[2] $\mu_B^{(2)}$	[3] $\frac{\mu_B .100}{\sqrt{\mu_B^2+\mu_L^2}}$ %	Girls. $\sqrt{\mu_R^2+\mu_L^2}$	$\mu_B^{(2)}$	$\frac{\mu_B .100}{\sqrt{\mu_B^2+\mu_L^2}}$ %
6	F	2.41	1.73	71.8	3.75	2.5	45.3
	W	3.55	2.43	68.4	3.47	2.25	64.8
	E	3.13	2.34	74.7	3.01	1.78	59.1
	S	3.62	2.58	71.2	3.61	2.43	67.3
7	F	3.5	2.57	73.4	4.06	3.35	82.5
	W	3.9	2.71	69.4	3.68	2.44	66.3
	E	4.83	2.54	52.5	3.61	1.78	49.3
	S	3.54	2.34	66.1	3.82	2.27	59.4
8	F	3.28	2.12	64.3	3.82	2.18	57.
	W	4.03	2.53	62.7	3.82	2.39	62.5
	E	3.68	2.40	65.2	3.55	1.84	51.8
	S	3.42	2.41	70.4	4.11	2.01	48.9
9	F	5.46	2.92	53.4	4.04	2.69	66.5
	W	5.17	3.36	64.9	4.39	2.55	58
	E	5.09	3.18	62.4	4.68	2.95	63.
	S	5.09	2.80	55.	4.83	2.55	52.7
10	F	3.86	1.62	41.9	4.28	2.99	69.8
	W	4.96	3.06	61.7	4.41	3.20	72.5
	E	4.5	2.76	61.3	3.91	2.78	71.
	S	3.89	2.39	61.4	3.82	2.19	57.3
11	F	4.44	2.59	58.3	4.88	2.84	58.1
	W	5.9	3.56	60.2	5.87	3.54	60.3
	E	4.81	2.92	60.7	4.95	3.26	65.8
	S	3.89	3.09	79.4	4.74	2.63	55.4
12	F	6.6	4.43	67.1	5.25	2.68	51.
	W	6.38	3.62	56.7	7.	3.08	44.
	E	5.52	3.09	55.9	6.72	2.64	30.3
	S	4.88	2.78	56.9	5.61	2.77	49.3
13	F	4.67	3.49	74.7	6.58	3.14	47.7
	W	5.16	3.32	64.3	7.75	3.46	44.6
	E	5.75	3.23	57.9	7.27	3.52	48.4
	S	5.89	2.53	42.9	5.51	3.01	56.4
14	F	5.23	3.33	63.6	4.17	3.33	58.7
	W	4.64	2.89	62.2	5.3	2.89	57.8
	E	5.84	4.07	69.6	4.48	3.50	78.1
	S	4.95	3.21	65.	4.59	3.40	74.2
15	F	5.31	3.94	74.1	4.67	2.91	62.3
	W	5.76	3.65	63.1	5.87	3.34	56.8
	E	5.16	2.78	55.8	5.51	2.42	43.9
	S	4.88	2.94	60.2	5.59	2.41	43.1
16	F	7.08	4.16	58.7	6.17	2.61	42.3
	W	7.01	4.32	61.6	6.26	2.13	34.
	E	4.86	2.94	60.4	5.83	3.66	62.7
	S	4.88	2.87	58.8	4.88	3.04	62.2

[1]This column is the *solid upper* line in bilateral charts. (III.)
[2]This column is the *dotted lower* line in bilateral charts.
[3]This column shows per cent. of actual bilateral asymmetry to its chance value.

The fact of a certain degree of bilateral asymmetry and of a certain degree of bilateral symmetry, and the degree of each, are thus shown for each pair of joints at each age.

Bilateral Symmetry and Asymmetry of Development.

If it should appear that μ_B changes from year to year in the same direction and in an approximately corresponding degree with $\sqrt{\mu_R^2+\mu_L^2}$ then it would be inferred that the causes which determine the fluctuations of individual variation determine also the fluctuations of bilateral asymmetry; otherwise stated, that corresponding right and left side joints in most individuals do not grow together, do not have corresponding acceleration and retardation of growth together. Tendency of the curve μ_B to move parallel with the curves $\sqrt{\mu_R^2+\mu_L^2}$ is so far proof of asymmetrical growth.

On the other hand, if it should appear that μ_B altogether refuses to fluctuate with $\sqrt{\mu_R^2+\mu_L^2}$ then it would be inferred that the causes which bring about fluctuations of individual variations do not affect the degree of bilateral asymmetry; otherwise stated, that corresponding right and left side joints in most individuals grow together; have corresponding acceleration and retardation of growth together, through whatever periods of individual variation. Tendency of the curve μ_B to move independently in direction of the curve $\sqrt{\mu_R^2+\mu_L^2}$ is so far proof of symmetrical growth.

If we determine the amount of yearly increase or decrease in the values of μ_B and of $\sqrt{\mu_R^2+\mu_L^2}$ for each joint, we have in each case 80 results, 40 for boys and 40 for girls. A comparison of the 80 plus or minus increments of μ_B with the 80 plus or minus increments of $\sqrt{\mu_R^2+\mu_L^2}$ gives the following results:

I. μ_B and $\sqrt{\mu_R^2+\mu_L^2}$ tend to vary in the same direction oftener than would be accounted for by chance. In 35 out of 80 comparisons, both increase; in 15 out of 80 comparisons, both decrease; i. e., in 50 out of 80 comparisons, both change in the same direction. There is, therefore, so far, bilateral asymmetry of development.

The probability from the totals is 62.5 per cent that the degree of bilateral asymmetry will be changed to some extent in the same direction as the given function of the individual variations. For finger, wrist and elbow, the probability is greater, viz.: 70%, or 20% more than an even chance.

2. Besides the 30 cases out of the total 80 in which μ_B and $\sqrt{\mu_R+\mu_L^2}$ do not vary in the same direction, there are 38 cases in which the latter varies more than the former. There

is, therefore, so far, symmetry of development of the two sides.

The probability is 72.5% that the given function of the individual variation will fluctuate more than the bilateral asymmetry; and the probability is 85% that the mean variation will fluctuate more than the bilateral asymmetry or will change in the opposite direction. The probability is 15%, or 35% less than an even chance, that in any given case the bilateral asymmetry will vary in the same direction and in a greater degree than the given function of the mean variation.

The following table gives the mean values of μ_B and of $\sqrt{\mu_R^2+\mu_L^2}$ for each joint of boys and of girls, together with the corresponding $\left(\frac{\Sigma v}{n}\right)$ fluctuation of yearly values from the mean. In each case the fluctuation is shown to be much greater for $\sqrt{\mu_R^2+\mu_L^2}$ than for μ_B.

TABLE XII.

		Finger	$\frac{\Sigma v}{n}$	Wrist	$\frac{\Sigma v}{n}$	Elbow	$\frac{\Sigma v}{n}$	Shoulder	$\frac{\Sigma v}{n}$
$\sqrt{\mu_R^2+\mu_L^2}$	Boys	4.53	1.13	5.13	.85	4.74	53.	4.44	.70
	Girls	4.60	.73	5.25	1.15	4.86	1.08	4.64	.59
μ_B	Boys	2.95	.79	3.22	.45	3.02	.36	2.72	.25
	Girls	2.86	.27	2.93	.45	2.79	.54	2.52	.32

The fact and the degree of each tendency are shown in the graphical representation of each pair of joints by the relative directions of the lines connecting successive values of μ_B and of $\sqrt{\mu_R^2+\mu_L^2}$. The tendency of the two curves to run parallel and the greater fluctuation of the upper curve are both evident. It is, therefore, to be concluded that there is partial, and only partial, bilateral asymmetry in the development of the rate of voluntary movement. Corresponding joints do not have exactly corresponding growth, but the correspondence is considerably closer than would be accounted for by chance.

Comparison of the Joints.

The order of the joints as regards rate is not the same at all ages. The following table shows the order at each age:

TABLE XIII.

Order		1	2	3	4	Order		1	2	3	4
6	E	4	0	0	0	7	E	4	0	0	0
	W	0	4	0	0		W	0	4	0	0
	S	0	0	3	1		F	0	0	4	0
	F	0	0	1	3		S	0	0	0	4
8[1]	E	3	0	0	0	9	E	3	1	0	0
	W	0	3	0	0		W	1	3	0	0
	F	0	0	3	1		F	0	0	3	1
	S	0	0	1	3		S	0	0	1	3
10	W	4	0	0	0	11	W	3	1	0	0
	E	0	4	0	0		E	1	3	0	0
	F	0	0	4	0		F	0	0	4	0
	S	0	0	0	4		S	0	0	0	4
12[2]	W	4	0	0	0	13	W	3	1	0	0
	E	0	3	0	0		E	1	3	0	0
	F	0	0	3	0		F	0	0	4	0
	S	0	0	0	4		S	0	0	0	4
14	W	4	0	0	0	15	W	4	0	0	0
	E	0	3	1	0		E	0	3	1	0
	F	0	1	3	0		F	0	1	3	0
	S	0	0	0	4		S	0	0	0	4
16[3]	W	4	0	0	0						
	F	0	3	0	0						
	E	0	0	3	0						
	S	0	0	0	4						

[1]E=W1. [2]E=F. [3]E=F.

The figures show the number of times, out of a possible 4, that the given joint stands in the order indicated. Thus at the age of 6, the mean rate of the elbow is highest in four cases,— boys R and L, and girls R and L.

TABLE XIV.

Table showing the number of times, out of a possible 44, each joint is found in each order as regards rate.

	Highest	Second	Third	Lowest	
Wrist	27	16	0	0	= 43
Elbow	16	20	5	0	= 41
Finger	0	5	32	5	= 42
Shoulder	0	0	5	39	= 44

Once E=W. Twice E=F.

The following table shows the amount by which elbow, wrist and finger severally exceed the shoulder at each age:

TABLE XV.

	Boys.				Girls.		
	Years	E–S	W–S	F–S	E–S	W–S	F–S
Right	6	3.7	3.4	–.3	2.8	1.7	–.1
	7	3.7	3.2	.5	3.0	2.9	.5
	8	3.8	3.8	.8	2.5	2.4	.3
	9	4.1	3.7	.3	2.7	3.2	1.3
	10	6.5	5.9	2.6	4.9	5.9	3.2
	11	5.6	6.4	2.9	3.7	5.5	2.2
	12	4.9	6.6	4.3	3.7	5.9	2.5
	13	5.6	6.8	3.2	3.7	5.7	2.8
	14	5.5	5.8	4.3	3.0	3.7	2.9
	15	4.9	7.9	5.3	2.2	4.9	3.1
	16	4.0	7.3	5.3	3.1	5.4	3.4
Left	6	2.2	1.5	–.2	1.8	1.0	.1
	7	2.1	1.4	.3	2.7	1.2	.3
	8	2.1	2.0	.3	1.4	.8	–.5
	9	2.9	2.7	.6	1.8	1.8	–.2
	10	4.2	4.2	1.9	2.4	2.5	.7
	11	4.5	4.4	2.4	2.6	2.7	1.3
	12	3.9	4.5	3.9	2.6	3.4	2.2
	13	3.8	3.9	2.4	3.4	3.4	1.5
	14	4.3	4.7	3.2	3.1	4.2	3.0
	15	4.0	5.1	3.8	2.2	2.8	1.3
	16	4.1	6.4	4.1	2.0	3.3	2.4

These results show that the shoulder grows most slowly, the elbow slightly faster, the wrist and finger very much more

rapidly. At 6, the finger joint is slowest, the elbow fastest; at 16, the finger has passed the elbow.[1]

LONGITUDINAL ASYMMETRY.

It is possible, by the method employed in determining the fact and degree of bilateral asymmetry, to determine whether there is any degree, and if so, what degree, of longitudinal asymmetry. The results for the right side in boys of 6, 9, 12 and 15 were treated in this manner, the remaining material being reserved for future treatment. The following table shows a comparison of the values of $\mu_{\text{Long.}}$ with the values of $\sqrt{\mu_{J\,1}^2+\mu_{J2}^2}$ for the ages named, finger and wrist, wrist and elbow, elbow and shoulder; finger and shoulder being so compared.

μ_J = mean variation of one of the joints compared.
$\mu_{\text{Long.}}$ = mean variation of individual values of (J_1-J_2) from their mean.

TABLE XVI.

TABLE SHOWING LONGITUDINAL ASYMMETRY.

		I	II	III
Joints Compared	Ages	$\sqrt{\mu_{J\,1}^2+\mu_{J\,2}^2}$	Long. Asymmetry $\mu_{\text{Long.}}$	$\frac{\text{II. }100}{\text{I.}}$ = % of Asymmetry
Finger and Wrist	6	3.25	2.15	.66
	9	5.43	2.94	.49
	12	7.61	4.05	.53
	15	6.	3.89	.65
Wrist and Elbow	6	3.08	2.72	.88
	9	4.97	2.52	.50
	12	6.69	3.50	.52
	15	5.81	4.02	.69
Elbow and Shoulder	6	3.16	3.25	1.02
	9	5.07	2.54	.50
	12	5.29	3.52	.66
	15	4.97	3.49	.70
Finger and Shoulder	6	3.33	3.15	.94
	9	5.54	4.21	.76
	12	6.34	4.04	.63
	15	5.19	4.36	.84

[1] For discussion, see page 201.

The fact of partial and only partial asymmetry of development, is thus shown to hold longitudinally as well as bilaterally. It will be observed that the degree of asymmetry approaches most nearly its chance value at age 6, and that in one instance at that age it is slightly in excess of its chance value. As in the bilateral comparisons, however, the absolute degree of asymmetry does not change much with age. The greatest asymmetry, absolute and relative, appears between finger and shoulder; the greatest correlation, between finger and wrist.

Boys and Girls.

It was shown on page 50 that the differences between boys and girls are slight. Within the narrow limits there indicated, however, there is a slight superiority of boys over girls. Of the 29 cases in which the difference between boys and girls is greater than $(\mu_{\circ B}+\mu_{\circ G})$,[1] 24 are in favor of the boys and 5 in favor of the girls. Of the remaining 59 cases, 33 are in favor of boys, 19 in favor of girls and 7 are the same in both. The superiority of the boys over the girls increases slightly from the age of 6 to the age of 9; and more decidedly from 14 to 16. They are nearest together at 10, 11 and 12. At 13 the girls are superior to the boys for each of the eight joints tested. It has been pointed out elsewhere that the period from 12 to 13 is a period of retardation of rate in boys and acceleration in girls. (Page 160, Table VIIa.)

The superiority of the boys' right side over the girls' right side is slightly greater than the superiority of the boys' left over the girls' left. On the right side, 16 out of 44 cases show $(B—G) > (\mu_{\circ B}+\mu_{\circ G})$. Upon the left side there are eight such cases out of 44.

From this fact it comes that there is a slightly greater difference between the right and left sides in boys than in girls. In 33 out of 44 cases [(r-l) boys compared with (r-l) girls] the difference between the right and left is greater for boys than for girls. For each joint the average difference between r and l is greater for boys than for girls. See page 162, Table VIII.[2]

Further light is thrown upon these relations by a study of the increase of rate in boys and girls. If the amount of in-

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[1] $\mu_{\circ B}$ = the mean variation of a mean rate made by boys.
$\mu_{\circ G}$ = " " " " " " " " girls.

[2] The value of (r-l) boys — (r-l) girls is small, as follows:
2 taps or over in 5 seconds, 2 cases.
Between 1 and 2 taps in 5 seconds, 15 cases.
Less than 1 tap in 5 seconds, 27 cases.

crease in rate for each year be found for each joint tested, the following facts appear:

On the right side the amount of yearly increase of rate is greater for boys than for girls in 24 out of 40 cases. On the left side the boys' rate increases more than that of girls in 20 out of 40 cases. This indicates that on the whole, the boys' right side improves slightly faster than the girls' right side, while the boys' left side improves no faster, possibly more slowly, than the girls' left side.

Bilateral Asymmetry in Boys and Girls.

Above it has been shown that the absolute difference between right and left is greater for boys than for girls, due to the more rapid development of the right side in boys. A study of the mean variation about the average (r–l) shows that there is a closer bilateral correlation in girls than in boys. If we represent by μ_B and μ_G the values of the mean variations about the average (r–l) values for each joint tested, of boys and girls respectively, a table of $(\mu_B - \mu_G)$ values shows the following results:

1. The value of $(\mu_B - \mu_G)$ is small, as follows:

In 2 cases out of 44, 2 taps or more in 5 seconds.
" 12 " " " " 1 " " " " " "
" 30 " " " " less than 1 tap in " "
" 31 " " " " the value of $(\mu_B - \mu_G)$ is less than the sum of the mean errors of the mean (r–l) values concerned.

2. In 12 cases out of 44 $(\mu_B - \mu_G) > (\mu_{oB} + \mu_{oG})$ and is plus, i. e., the asymmetry is greater for boys than for girls.

In 1 case out of 44 $(\mu_B - \mu_G) > (\mu_{oB} + \mu_{oG})$ and is minus i. e., the asymmetry is greater for girls than for boys.

In 19 of the 31 cases in which $(\mu_B - \mu_G) < (\mu_{oB} + \mu_{oG})$, the value of $(\mu_B - \mu_G)$ is plus. In 10 cases this value is minus. In 2 cases it is 0. In the case of every joint, the average bilateral asymmetry is greater for boys than for girls.

This greater bilateral asymmetry in boys is affected by the varying rapidities of growth in the two sexes. For example, from 9 to 10 and from 12 to 13 are clearly marked periods of retarded growth of rate for boys. In these years, the difference in symmetry between boys and girls is reduced practically to zero. In the years from 7 to 9, or from 15 to 16, on the contrary, especially in the latter period, the greater bilateral asymmetry of boys is clear.

It was pointed out (Page 161) that there is a decline of rate in girls from 13 to 14, and in boys from 14 to 15, that these periods are preceded by a year of accelerated growth, and are followed by more or less rapid recovery. It is significant

that the decline and the antecedent acceleration are more extreme in girls, and that the recovery is slower. In proof:

A comparison of the rates of girls at 13 with the rates of girls at 16, shows that the former almost reach and in three cases surpass the latter.

Fifteen of the twenty-five individual rates of 8 per second or over, made by girls, were made by girls of 13.

Although, as shown elsewhere, the rate of girls is generally slightly less than that of boys,—at the age of 13, every joint shows a higher average in girls than in boys; and in the case of four joints, the girlsof 13 are faster than the boys of 14.

The decline is greater in the case of girls.

Comparison of the retardation of rate in boys from 14 to 15 with that in girls from 13 to 14, shows the latter to be greater in the case of seven of the eight joints. The same facts appear graphically in the rate charts.

The girls recover more slowly.

Comparison of the retardation of rate in boys from 14 to 15 with that in girls from 13 to 14, shows the latter to be greater in the case of seven of the eight joints. The same facts appear graphically in the rate charts.

Comparison of the increments of rate in boys from 15 to 16 with those in girls from 14 to 15, shows the former to be decidedly greater in the case of every joint; and in the case of seven of the eight joints, the increment of rate in boys from 15 to 16 is greater than that in girls from 14 to 16.

Note on Results from Left-handed Children.

The small number of left-handed subjects at any one age prevents much profitable comparison of these records with those from right-handed subjects. In 11 out of 80 (10 yrs., 4 joints, 2 sexes) cases (14%), the mean rate of right-hand joints is greater in left-handed than in right-handed subjects; in 55 out of 80 cases (69%), the left-hand joints of left-handed subjects are faster than the corresponding joints of right-handed subjects; in 66 out of 160 cases (41.2%), the mean rate of joints in left-handed subjects is faster than that of the corresponding joints in right-handed subjects. These percentages are only to be taken as rough approximations.

The fact that values of (r-l) do not fluctuate so much at different ages as to make them incomparable, has caused me to calculate the mean (r-l) for each joint of the 26 boys and of the 20 girls irrespective of age. The results (Table XVII.) show that the average difference between r and l is very small compared with the difference in right-handed subjects, and that notwithstanding the heterogeneity of age the bilateral asymmetry is generally smaller than in right-handed subjects.

XVII.

Table showing mean values of (r-l) in left-handed subjects and degree of bilateral asymmetry.

	Av. (r-l)	$\frac{\Sigma v}{n}$		Av. (r-l)	$\frac{\Sigma v}{n}$
Boys F	1.2	3.3	Girls	—.2	2.4
W	—.1	2.7		.1	3.
E	—.8	2.1		.8	3.1
S	—.2	2.1		—.2	.95

Review of Facts on Rate of Voluntary Movement.

1. The maximum rate of rhythmically repeated voluntary movement is subject to changes in a given individual which are usually slight and gradual. (P. 142.)

2. These changes are sufficient in amount and in constancy to indicate, surely, local and general subjective conditions, as excitement, general and local fatigue, local cold, and the improvement with age. (P. 144 *et seq.*)

3. The change of rate with extreme fatigue is large in comparison with the mean rate of improvement with age. (P. 148.)

4. The amplitude of movement may be changed within wide limits without affecting the rate. (P. 150.)

5. The mean rate of growth of rate between ages 6 and 16 ranges from .15 to .3 taps per second in various joints. (P. 159.)

6. The rate of growth of rate ability is not uniform. Well marked periods of accelerated and of retarded growth appear. (P. 160.)

7. The mean rate of a right side joint, for a group of right-handed subjects, is always higher than that of the corresponding left side joint. The probability that the right will exceed the left in any case chosen at random is about 80%. (P. 161 *et seq.*)

8. The mean rates of corresponding right and left side joints both increase or both decrease in about 90% of cases.

9. Right side joints are subject to slightly greater plus and minus fluctuations of rate ability than are left side joints. (P. 162, 163.)

10. Right side joints gain little if any in rate ability, more than do left side joints. (P. 163.)

11. There is partial and only partial asymmetry of development, bilateral and longitudinal. (P. 163–169, 172.)

12. The hand outgrows the arm between the ages here examined. (P. 170 *et seq.*)

13. The mean rate of boys slightly exceeds that of girls at all ages, except where retardation of growth in boys coincides with acceleration of growth in girls. (P. 173.)

14. There is less bilateral asymmetry of development in the rate ability of boys than in that of girls. (P. 174.)

15. The left-handed persons examined show decidedly less mean difference between right and left and less bilateral asymmetry than do right-handed persons. (P. 175.)

PRECISION OF VOLUNTARY MOVEMENT AS REGARDS FORCE AND DIRECTION.

I.

There is no lack of sufficiently delicate qualitative tests of precision of movement. Personal carriage, speech, games, industrial occupations, scientific technique, fine arts,—in short, all forms of active life afford a multitude of such tests, by means of which the degree of muscular control or lack of it is more or less accurately estimated.

For the clinical determination of precision of movement, besides taking notice of visible irregularities of muscular control and of irregularities shown in any of the subject's ordinary work or play, the following special devices have been used. The patient tries:

1. To draw a straight line ([13]).
2. To write his name or other words ([14]), ([15]).
3. To touch suddenly a specified spot with the point of a pencil ([16]).
4. To hold a reed attached to the finger still, in position to write upon, ([13]) or to cast a shadow upon, ([17]) the revolving drum.
5. To apply constant pressure to some form of dynamograph ([18]), ([19]), ([20]).

All these devices test the control of amount of force exerted, the dynamograph doubtless best. All except the dynamograph test also particularly the control of the direction in which force is exerted. All give or may furnish material for a graphic record of results; and it is not impossible to work out from any of them, with sufficient labor, a numerical result. It is doubtful whether in practice any one seeks to get a numerical result from any of these devices, except the third, and in that case "the result is hardly worth the trouble." (Gowers I. 5.)

II.

The idea has presented itself that precision of movement as dependent upon control of the amount and direction of force may be accurately and conveniently measured, giving a numerical and if desired a graphic result, by a variety of devices the essential point of which is as follows: To one pole of a battery is attached an apparatus which presents a series of spaces, graded in size as finely as desired, and bounded by the conducting medium; to the opposite pole is attached some appropriate form and size of stylus. Or, the stylus may vary in size, the open space in the other electrode being of some appropriate form and size. The task in either case is to determine within what limits of precision either or both of the electrodes may be moved or held still without making contact. The numerical result is read from the instrument. A graphic result can alway be readily constructed from the numerical, and in some forms of apparatus to be described, may be made by the subject.

III.

Five forms of apparatus were made upon this principle, adapted to test various muscles and movements. In the experiments here reported, two of these forms were used. The first of these, Fig. 2, is essentially a device for measuring the precision shown in drawing a straight line. Upon a smooth and hard surface (A) (e. g., glass) were fastened two strips of platinum-foil (B) so that they formed an acute angle 2°-2½°

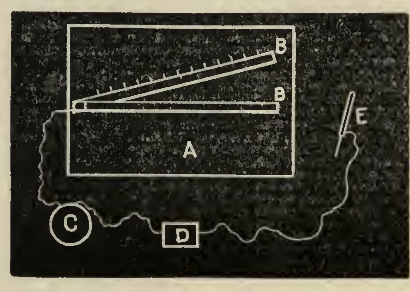

Fig. 2.

A—Plate Glass. *B*—Strips of foil. *C*—Battery. *D*—Sounder. *E*—Stylus.

with each other. The platinum was connected with one pole of a 1 to 4 cell battery (C); to the other pole was attached by flexible wire an ordinary steel pen or needle E. Required to draw a straight line between the arms of the compass as near

as possible to their intersection without making contact.[1] The distance between the arms at the point of contact (minus the thickness of the stylus) is, in general, a measure of deviation from the straight line at that point. The angle at which the arms of the instrument are set is not essential, since chords of every absolute length may be found, at some radius, in every angle; but the angle may be varied for various subsidiary purposes. A little calculation will determine for a given angle, the chords corresponding to each unit of length. Or since radius and chord are functions of each other the direct readings give at once the relative accuracy.

IV.

Preliminary Test of Precision.

In Writing Movement.

A number of conditions aside from the subjective condition of the person experimented upon, appear from prefatory trials to affect the accuracy of the result. (1) The position of the instrument relative to the body, (2) the muscles employed, (3) the support of the muscles employed, (4) the distance moved, affecting variously readjustments of the muscles while in motion, and (5) the rate of motion—all appear to affect the result. In order to make a beginning, even at the determination of these variables, it is necessary to prescribe conditions in at least four of the five. In the prefatory set of experiments with apparatus No. 1, the following conditions have been prescribed: The subject is seated before the table on which the apparatus rests; the forearm and hand are supported by the table; the instrument lies at an angle of about 50° to the right of a perpendicular to the frontal plane, now with the apex of the angle away from the subject and now with the apex of the angle towards him; the subject begins the writing movement 30 mm. from the zero point and moves continuously at any rate he pleases; the angle between the arms of instrument was in the experiments with adults 2°, in the experiment with school children 2½°.

It is believed that the rates, instinctively chosen by the subject, may in the outset be taken in preference to any prescribed rate.[2]

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[1] The zero point is that point at which the stylus just makes contact with both arms of the compass.

[2] See Camerer's conclusion that the natural rate of movement is not a constant, but a gradually accelerated one. P. 138.

Method of Treating Results.

If a be the angle between the arms of the instrument, and l the distance of any point along the scale from the zero point, then half the distance between the arms of the instrument at any point is l sin. $\frac{1}{2}$ a. If t equal the number of trials at the point l, and h equal the number of hits, then $\frac{h\,100}{t}$ is the empirically determined probability in per cent. of a deviation, $l\ sin.\ \frac{1}{2}\ a$.

If we assume $P_{l\ \sin.\ 1\text{-}2\ a} = \frac{2}{\sqrt{\pi}} \int_0^{h\ l\ \sin.\ 1\text{-}2\ a} \varepsilon^{-t^2}\, dt$, the value of $h\ l\ sin.\ \frac{1}{2}\ a$ corresponding to the ascertained $P_{l\ \sin.\ \frac{1}{2}\ a}$ can be obtained from the table of values of the Probability Integral

$$\frac{2}{\sqrt{\pi}} \int_0^t \varepsilon^{-t^2}\ dt.$$

for argument t. The value of l and of $sin.\ \frac{1}{2}\ a$ are known and h can be obtained from $h = \frac{t}{l\ \sin.\ 1\text{-}2\ a}$.

The value of h, the measure of precision, may be determined in this way for as many different points along the scale as desired. In this work, the determination was made for every millimeter of the scale where there were trials and touches. For points at which no touches were made, of course, no percentage of touches to trials could be obtained.

The degree of precision was thus determined for each set into which the results obtained were classified.

It is not assumed by the foregoing calculation that the value of h is the same for different parts of the scale. The contrary is found to be the fact. This is brought out more clearly by taking the weighted mean of the values of h for each millimeter from 1 to 5, then of the values of h from 6 to 10, and so on in groups of 5 throughout the scale. By weighted means in this case it is meant that each value of h is multiplied by the number of trials made at that point; and that the sum of these products for the several points in the group is divided by the sum of the trials made at the several points in the group. These weighted means represent approximately the average precision of the class for that part of the scale. The values of h thus found are of course abstract numbers, and are significant only in comparison with each other. It seems desirable to know within what limits, in terms of ordinary standards of length, the deviations from a straight line fall. To find within what limits a certain per cent. of cases, say 68.3%, is likely to fall at the point for which the precision is h :

In general,

$$(h^1 x^1) : (h^1 x) : : x^1 : x$$

Where $(h^1 x^1)$=value of t from the Probability Integral table,

x^1=the known value of l sin. ½ a

and $h^1 x$=.683.

$$\therefore \quad x = \frac{.683.\ l\ \sin.\ 1\text{-}2\ a}{h^1\ l\ \sin.\ 1\text{-}2\ a}.$$

Since all the quantities on the right are known, the value of x is obtained by carrying out the indicated operations. This determination was made for the weighted mean of the values of h, for each 5 millimeters of the scale, in the case of each set of results obtained. The tables give in fractions of a millimeter twice the distances within which 68.3% of all deviations from the central line occur, and the weight or number of trials which entered into the determination of each.[1]

The object of this very laborious procedure was to ascertain and take account of the behavior of the pen point in every millimeter of its course and so to increase many fold the information to be obtained from each trial. For example, W. B. made 810 attempts to carry the pen point from 30 mm. to 0 mm. without touching. If the record of these attempts gave information only in respect to one point, namely, the point of touching, the sum of the weights of our information from the experiments could be only 810. But since by the foregoing method it is possible to take account of the average behavior of the pen point at nearly every millimeter of the

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[1] Details of the calculation:

Column I. Represented each mm. along the scale from 0-30 mm.
II. The number of touches recorded at each mm.
III. The number of trials made at each mm.
IV. The per cent. of touches to trials at each mm.
V. The per cent. of successes to trials at each mm.
VI. The value of (*h l sin.* ½ *a*) corresponding to the per cent. in V., obtained from the integral table.
VII. The values of (*h sin.* ½ *a*) obtained by dividing by the *l* from column I. Since (*sin.* ½ *a*) is constant these numbers are a measure of precision.
VIII. The products of the numbers in VII., by the corresponding numbers in III.,—i. e., precisions multiplied by their respective weights.
IX. The sums of the numbers in VIII., in groups of five.
X. The quotients of the numbers in IX., by the sums of the corresponding numbers from III. That is, x gives the weighted values of (*h sin.* ½ *a*) for each 5 mm. of the scale.
XI. From each of these, by the method described, page 83, the limits within which 68.3% of all deviations occur were determined. These numbers are given in the tables in connection with their respective weights obtained from column III.

scale, the numerical weight of information about the behavior of the pen in W. B.'s hand is 10,712.[1]

Eight (University) adults were tested in the manner described. Table XVIII. gives the number of trials made by each, the mean distances from the 0 point reached by each and the corresponding individual variations.

Table XIX. gives the results by the method explained page and shows accordingly the breadth of space in fractions of a millimeter within which 68.3% of all deviations fall. The numbers given are twice the deviation in one direction. The number before the colon is in each case the weight of the mean following the colon; thus 465 is the weight of the result .12 mm.

TABLE XVIII.

EXPERIMENTS ON PRECISION, WRITING MOVEMENT.

Persons	No. of Trials	Downward Movement		No. of Trials	Upward Movement	
		Av.	M. V.		Av.	M. V.
J. L.	50	9.6	2.7	50	11.1	3.6
J. A. B.	50	8.7	3.4	50	9.6	2.8
E. C. S.	50	8.2	3.1	50	6.	2.
F. B. D.	75	5.1	2.9	75	5.3	2.7
W. B.	100	5.	2.7	100	8.9	3.2
A. F.	50	4.7	2.1	50	5.3	1.9
T. L. B.	50	6.1	2.7	50	5.3	1.6
L. B.	75	3.9	1.9	75	4.2	1.6

Total number of trials, 1000.

[1] I again heartily acknowledge my indebtedness to Dr. Franz Boas for valuable advice, and at the same time must free him from all responsibility for the method which I have adopted.

TABLE XIX.

Part of Scale		I 1—5	II 6—10	III 11—15	IV 16—20	V 21—25	VI 26—30
		W't mm	W't mm	W't mm	W't mm	W't mm	W't mm
Subject L. B.	R Down	465 : .12	931 : .11	194 : .22			
	R Up	411 : .16	1057 : .12	443 : .16			
Subject T. L. B.	R Down	58 : .19	188 : .14	157 : .19			
	R Up	58 : .18	223 : .15				
Subject A. F.	R Down	96 : .12	217 : .14	50 : .17			
	R Up	60 : .16	201 : .15	144 : .19			
Subject F. B. D.	R Down	146 : .12	317 : .13	141 : .16	72 : .27	74 : .38	
	R Up	124 : .11	306 : .14	286 : .19		74 : .34	
Subject J. A. B.	R Down	24 : .22	156 : .19	192 : .22	56 : .29	59 : .41	
	R Up	6 : .39	141 : .22	143 : .21	159 : .26	116 : .31	
Subject E. C. S.	R Down	21 : .33	161 : .18	131 : .20	95 : .24	49 : .33	50 : .39
	R Up	65 : .16	222 : .14	150 : .26			
Subject J. L.	R Down		115 : .24	157 : .21	132 : .27		
	R Up		73 : .27	172 : .26	46 : .30	144 : .32	50 : .38
Subject W. B.	R Down	290 : .17	1470 : .19	1969 : .17	1626 : .19		
	R Up	244 : .16	1247 : .20	1872 : .20	1994 : .21		

1. Under the conditions named, the mean deviation varies in the eight individuals from about $\frac{.12}{2}^{\text{mm}}$ to about $\frac{.38}{2}^{\text{mm}}$ (×).

(×) That is, the deviations in one direction are one-half the numbers given in the table.

2. The mean variation of individual trials from the means ranges from about 30% to about 60% of the means.

3. It appears in general from Table XVIII. that the greater means go with the greater variations, i. e., the device shows the more accurate person, both by the relatively greater mean inaccuracy and by the greater irregularity in successive trials.

4. Owing to the fact that the maximum time required by any subject for the trials made at one time was never over five minutes, and to the fact that the writing movement is so thoroughly habituated, fatigue was practically excluded.

5. W. B. made about 800 trials, extending over a period of three weeks, without showing an observable improvement from practice.

School Tests on Precision.

Writing Movement.

With a few exceptions, the same pupils tested in the rate experiments were tested for precision.

Apparatus: Strips of platinum foil were pasted smoothly on plate glass so as to make an angle of 2½ degrees. A small steel needle, set in a common wooden penholder, served as stylus. Three to four Le Clanche cells constituted the battery. A telegraph sounder gave the signal when the needle touched the platinum.

Course of the Experiments: The child was seated in front of the table where the glass plate lay. The latter was placed in such a position that the line along which the stylus was to be drawn should make an angle of 45°—50° with the frontal plane of the subject, when the right hand was used; and an angle of 130°—135°, when the left hand was to be used. The stylus was in each case placed between the arms of the instrument, 30mm. from the 0 point, and drawn toward the 0 point until contact was made. Six trials were made with each hand, three movements in each case being made away from the body, and three toward the body. The point of contact was recorded. The child learned what he was expected to do as follows: (A.) In nearly every case he looked on while one or more of his comrades went through the tests. (B.) He received plain directions, e. g., "Take the pen; hold it so; put the point here; draw the pen so, without touching either side," etc. (C.) He was made to try several times, until it was quite certain that he knew what he was expected to do. In some cases with very small and very stupid children, it proved impossible to make them understand what they were expected to do. Such cases, after long and patient trial, were abandoned. But records were not excluded because of being unusually inaccurate, if it could be ascertained from the child's answers and efforts that he knew what to try to do. The entire series of precision tests in the schools of Worcester was taken with scrupulous attention to every detail by my wife, Mrs. Lotta Lowe Bryan.

Classification of Results.

The results were classified according to the age and sex of the subjects, according to the hand used, and according to the direction of movement. Each class of results was treated by the method described, page 180. The Table XX. gives the results. The numbers before the colons give the numerical weights of the results after the colons; the latter give twice the distance in mm. within which 68% of deviations fall. Results with a weight of 150 or more are printed in heavy type.

Table XX.

School Tests on Precision.

Part of Scale		I 1—5	II 6—10	III 11—15	IV 16—20	V 21—25	VI 26—30
	Age	W't mm	W't mm	W't mm	W't mm	W't mm	W't mm
	6		17 : .69	152 : .45	304 : .44	229 : .43	
	7		62 : .40	237 : .38	424 : .41	302 : .39	
	8	15 : .23	168 : .30	399 : .30	490 : .33		
	9	49 : .16	283 : .26	508 : .30	610 : .31		
Boys	10	45 : .25	251 : .26	472 : .29	214 : .35		
Right	11	84 : .14	305 : .24	504 : .26	108 : .25		
Down	12	61 : .17	247 : .26	423 : .28	285 : .27		
	13	153 : .17	401 : .19	294 : .21	203 : .28		
	14	173 : .14	433 : .20	571 : .23	238 : .27		
	15	71 : .17	293 : .24	455 : .25	96 : .29		
	16	48 : .27	256 : .23	366 : .25	78 : .28		
	6		25 : .54	155 : .44	326 : .43	154 : .38	
	7		72 : .45	263 : .38	448 : .40	203 : .36	
	8	10 : .26	163 : .33	370 : .34	196 : .32		
	9	39 : .16	246 : .29	524 : .31	374 : .28		
Boys	10	22 : .23	208 : .31	466 : .31	214 : .29		
Right	11	45 : .27	288 : .27	504 : .27	108 : .30		
Up	12	70 : .17	302 : .24	366 : .26			
	13	80 : .16	333 : .25	392 : .24			
	14	118 : .16	421 : .22	571 : .22	120 : .25		
	15	120 : .19	398 : .20	191 : .22			
	16	97 : .19	323 : .22	231 : .21			
	6			62 : .59	171 : .54	305 : .54	78 : .53
	7			52 : .50	218 : .57	411 : .52	297 : .52
	8		31 : .52	165 : .41	358 : .48	476 : .41	99 : .42
	9		59 : .36	215 : .45	484 : .45	609 : .41	
Boys	10		73 : .42	248 : .38	462 : .42	320 : .38	
Left	11	4 : .66	83 : .35	253 : .36	463 : .44		
Down	12	9 : .20	114 : .33	320 : .37	462 : .34		
	13	33 : .32	200 : .24	360 : .39	381 : .35		
	14	13 : .20	184 : .30	418 : .33	556 : .34	239 : .34	
	15	47 : .19	263 : .25	412 : .27	282 : .31	96 : .34	
	16	31 : .30	194 : .24	310 : .24	228 : .33		
	6		10 : .63	57 : .48	166 : .56	289 : .56	231 : .53
	7		43 : .36	128 : .40	283 : .49	427 : .48	299 : .51
	8		42 : .40	181 : .40	342 : .47	453 : .45	201 : .51
	9	8 : .26	104 : .32	287 : .37	498 : .45	615 : .41	129 : .44
Boys	10	26 : .20	135 : .26	295 : .35	464 : .39	318 : .41	
Left	11	11 : .20	93 : .37	298 : .34	473 : .40	214 : .36	
Up	12	38 : .16	122 : .29	328 : .35	458 : .36	96 : .35	
	13	58 : .17	223 : .22	377 : .29	471 : .34	302 : .37	
	14	94 : .16	263 : .22	460 : .32	572 : .32	120 : .33	
	15	56 : .17	242 : .26	406 : .27	575 : .30		
	16	59 : .19	219 : .22	348 : .28	231 : .29		

Table XX.—*Continued.*

School Tests on Precision.

Part of Scale		I 1—5	II 6—10	III 11—15	IV 16—20	V 21—25	VI 26—30
	Age	W't mm	W't mm	W't mm	W't mm	W't mm	W't mm
	6	3 : .48	56 : .29	212 : .43	387 : .40		
	7		41 : .53	224 : .42	399 : .40	274 : .39	
	8	21 : .24	182 : .23	352 : .32	359 : .33		
	9	24 : .21	171 : .27	394 : .32	506 : .31		
Girls Right Down	10	35 : .22	243 : .24	449 : .31			
	11	89 : .18	344 : .24	487 : .27			
	12	74 : .16	235 : .22	422 : .30	500 : .30		
	13	53 : .21	265 : .26	463 : .26	102 : .28		
	14	92 : .13	304 : .22	355 : .24	378 : .30		
	15	56 : .21	294 : .22	330 : .27	366 : .30		
	16	15 : .31	142 : .26	188 : .28			
	6		79 : .34	218 : .43	307 : .43	83 : .41	84 : .43
	7		82 : .36	265 : .39	427 : .37	185 : .35	
	8	17 : .31	169 : .29	363 : .29	361 : .32		
	9	26 : .30	191 : .27	411 : .34	313 : .32		
Girls Right Up	10	38 : .27	294 : .34	476 : .25	203 : .28		
	11	48 : .24	306 : .27	506 : .25			
	12	55 : .25	283 : .25	453 : .27	303 : .27		
	13	80 : .18	341 : .23	390 : .24			
	14	86 : .20	354 : .22	460 : .22	96 : .29		
	15	75 : .22	321 : .23	264 : .23	93 : .29		
	16	69 : .15	213 : .22				
	6		4 : .50	82 : .48	202 : .50	356 : .57	84 : .45
	7		8 : .35	122 : .45	287 : .49	434 : .48	93 : .42
	8		25 : .34	147 : .48	365 : .48	275 : .40	
	9		33 : .43	172 : .43	383 : .51	318 : .42	
Girls Left Down	10		42 : .36	176 : .44	413 : .47	203 : .38	
	11		79 : .31	276 : .41	465 : .38	465 : .38	105 : .40
	12	3 : .21	72 : .34	253 : .40	440 : .43	203 : .34	
	13	9 : .74	146 : .30	351 : .35	482 : .34		
	14	7 : .57	169 : .31	348 : .31	447 : .32	191 : .41	
	15	28 : .22	176 : .24	323 : .32	427 : .34	184 : .39	
	16	6 : .48	122 : .27	212 : .32	150 : .33		
	6		23 : .40	83 : .44	202 : .54	370 : .55	84 : .48
	7		7 : .49	86 : .40	265 : .53	343 : .47	
	8	9 : .34	62 : .27	208 : .42	348 : .42	447 : .44	
	9		56 : .40	208 : .39	401 : .42	505 : .45	108 : .41
Girls Left Up	10	17 : .27	102 : .28	239 : .35	421 : .42	298 : .42	
	11	57 : .13	178 : .23	338 : .32	451 : .37	410 : .39	
	12	34 : .22	148 : .26	347 : .32	465 : .34	303 : .39	
	13	23 : .12	136 : .28	335 : .34	470 : .36	203 : .39	
	14	17 : .21	146 : .32	316 : .33	342 : .35	281 : .38	
	15	24 : .18	169 : .27	348 : .32	345 : .32	366 : .37	
	16	21 : .27	124 : .25	222 : .27	99 : .32	31 : .33	

Trustworthiness of Results.

1. Owing to the fact that the arithmetical mean of the individual results was not determined, it is not possible to give in the ordinary way the mean individual variation. In order to exhibit, however, the relative individual variation, I have determined for each of the 88 sets of results, the smallest distance along the scale within which two thirds of the individual results fall. In the following table these values, each divided by 2, are given as approximate measures of the individual distribution. It will be observed that the individual variation is greater for the left hand than for the right, and tends to decrease with advancing age.

TABLE XXI.

Table of values of smallest distance along the scale within which two thirds of hits fall, in mm.
2

	6	7	8	9	10	11	12	13	14	15	16
B. R. D.	4.	4.5	3.5	3.9	3.7	3.6	4.1	2.9	3.3	3.2	2.9
B. R. U.	4.5	4.	3.8	3.5	3.	3.1	3.2	3.	3.2	2.6	2.8
B. L. D.	5.4	4.5	4.2	4.3	4.3	4.4	3.8	4.9	4.	4.	4.1
B. L. U.	5.4	5.4	5.3	5.2	5.2	4.5	4.5	5.2	5.2	4.	3.9
G. R. D.	3.9	3.5	4.5	4.	3.8	3.5	4.6	3.5	3.6	3.6	3.
G. R. U.	4.	3.9	3.8	4.2	2.7	3.	3.6	3.	2.7	3.1	2.7
G. L. D.	4.6	4.8	3.5	3.8	3.9	4.	3.9	3.9	4.	5.2	3.4
G. L. U.	5.1	4.2	4.	4.2	5.5	6.3	5.	4.6	4.5	4.	3.5

2. A method of determining within certain limits the probable trustworthiness of results is afforded by a comparison of results from boys and from girls.

It appears that the maximum difference between a result for boys and the corresponding result for girls is .09 mm.; that the mean difference between boys and girls for the right hand is .004 mm., and for the left hand .007 mm.; and that 68.3% of the differences do not differ from the mean, for the right hand more than .02 mm., and for the left hand more than .029 mm.

This calculation is analogous to that for determining the mean variable error after the constant error has been found.

These numbers then, .02 mm. for the right hand and .03 mm. for the left hand, are measures of the probable mean variation of the results given in the table.

These numbers are unfortunately not so insignificant as may at first appear. The total reductions in the mean deviation between 6 and 16 are for the right hand about .23 mm. and for the left hand about .32 mm. This would show the average yearly reduction of the mean deviation to be for the right hand about .023 mm., and for the left hand about .032 mm. An examination of the table of actual yearly reductions of the mean deviations shows that the yearly plus or minus change in the mean deviation ranges from 0 to .098 mm. for the right hand, and from 0 to .085 mm. for the left hand. It appears, therefore, that in many cases the limit of doubt attaching to the results is greater than the change by growth in one year, and in some cases considerably more.

Although three times as many experiments were made upon an individual in each set of precision tests as in the rate tests, and although by the method of treatment each single trial by the pupil furnished information as to the precision at many points in its course, so that the numerical weights of the precision results are many times greater than the numerical weights of the rate results,—yet the former are still insufficient to define clearly the amounts of yearly growth. This result, which evidently comes from the much greater variability in the power to make precise movements than in the power to move at a maximum rate, is itself significant; but it limits greatly the possibility of deriving trustworthy conclusions concerning the development of precision of movement. In the following, only those conclusions will be given which stand apparently clear of doubt, in connection with all information possessed by the author for determining the several degrees of probability.

Variation in the Precision at Different Parts of the Scale.

It may be observed in almost every set of results that the mean precision increases as the apex of the angle is approached. This does not, of course, mean that fewer touches per hundred trials are made, but fewer, in proportion to the space between the arms of the instrument. It seems probable that this is due to the more perfect concentration of attention as the task becomes more difficult. In all comparisons made in the following treatment, as, for example, between right and left, or as between one age and another, the results obtained within the same 5 mm. of the scale are compared.

Extreme Limit of Variation.

The total reductions in the mean deviations between ages 6 and 16 are as follows: Boys, right, down, .26 mm.; boys,

right, up, .24 mm.; girls, right, down, .21 mm.; girls, right, up, .21 mm.; boys, left, down, .33 mm.; boys, left, up, .34 mm.; girls, left, down, .33 mm.; girls, left, up, .28 mm. In each case the results are given in terms of 2x, where x is the mean deviation in one direction from the straight line which bisects the angle *a*.

Yearly Variation.

To determine the yearly gain or loss in precision as measured by the mean deviations (Table XX., pp. 186, 187): Subtract each result with a weight of 150 or more from the result just above it in the table; e. g., in table, B. R. D., subtract each result for age seven from the result for age six which falls in the same group (.38 from .45, .41 from .44, and .39 from .43). Take the mean of these differences. Proceed in like manner with the results for ages 7 and 8, 8 and 9, etc. This calculation was made for all the results in Table XX. No conclusion is drawn from these results which is not justified by the several individual results.

The most obvious fact which appears is the great gain made between ages 6 and 8. This can be shown by placing side by side the gain in those years and the total gain from 6 to 16.

TABLE XXII.

	Boys.				Girls.			
	R. Down	R. Up	L. Down	L. Up	R. Down	R. Up	L. Down	L. Up
Reduction of Mean Deviation between 6–8.	mm. .128	.096	.098	.096	.090	.125	.095	.115
Reduction of Mean Deviation between 6–16.	.26	.24	.33	.34	.21	.21	.33	.28

Turning to the records from 12 to 16, I am unable to draw any conclusion as to the effect of the physiological changes in that period upon the degree of precision, except that the effect is too small to appear clearly from the amount of data possessed.

R. VERSUS L.

In the case of 305 individuals (boys and girls of 6, 9, 12, 15 and 16), each individual record was examined with reference to the superiority of the right hand over the left. Upward movements with the right hand were com-

pared with upward movements with the left hand, and likewise downward with downward. The six right hand records for each individual were paired with the six left hand records by taking the numbers in the order in which they stand in the original records ; i. e., the pairing was determined by chance. The following table shows the result of this comparison :

TABLE XXIII.

Age	No. of Comparisons.	R. Superior +	Equal. 0	R. Inferior —	% +	No. of Persons in whom R. was Always Best.
6 Boys,	153	123	3	30	78.8	9
Girls,	168	130	4	34	77.4	7
9 Boys,	258	215	6	37	83.3	20
Girls,	216	168	9	39	77.7	5
12 Boys,	192	154	6	32	80.2	9
Girls,	204	167	4	33	81.3	8
15 Boys,	192	135	15	42	70.3	1
Girls,	186	126	18	42	67.7	7
16 Boys,	156	101	8	47	64.7	3
Girls,	102	72	6	24	70.6	3
Total,	1830	1391	79	360	76.	72 = (23 + %)
Persons,	305					

The table shows (A) that only in the case of 72 individuals out of 305 (23 %) does every right hand trial exceed in precision the left hand trial with which it is compared. (B) That in 1,391 out of 1,830 comparisons (76%) the right hand result is superior to the left. (C) That the per cent. of advantage by the right hand is less at 15 and 16 than at 6, 9, or 12 years of age.

A comparison of the mean values given in the right hand tables, pages 88 +, with those from the left hand tables shows in 78 cases out of 83 the right hand superior to the left.

The amount of superiority of right over left varies remarkably with age. If the values of (r–l) be determined by sub-

tracting each r value in the table, with a weight of 150 or more, from the corresponding l value, and if the mean of these (l–r) values be determined for each age, it appears that for both directions of movement, for boys and for girls, there is a decrease in the value of (l–r) with age. If the mean difference between r and l for each age be determined (including in the mean the results for both directions of movement, and for both sexes), we have:

Average Superiority of R. over L. in terms of x. mm.

Age.	6	7	8	9	10	11	12	13	14	15	16
Av. x,	.122,	.12,	.114,	.101,	.097,	.09,	.087,	.06,	.07,	.038,	.025

Boys and Girls.

An examination of results on page 188 shows that with either hand, the boys are very slightly superior to the girls in precision.

Mean superiority of boys' right hand,	.004 mm.
" " " " left "	.007 mm.

In 47 comparisons of right hand results the boys are superior 24 times; girls 13 times; boys and girls equal 10 times.

In 52 comparisons of left hand results boys are superior 27 times; girls superior 22 times; boys and girls equal 3 times.

In the 99 comparisons, boys were superior 51.5%; girls superior 35.3%; boys and girls equal 13.2%.

Precision Experiments.

Probing Movement.

Gowers (13) quotes Blix as proposing to determine the degree of incipient ataxia by requiring the patient to tap several times with a pencil, endeavoring each time to strike a fixed point on the paper. The distances of the points actually struck from the fixed point furnish material for estimating the degree of ataxia when compared with results from normal individuals.

The objection to this method on account of the great labor involved (Gowers I. 5), may be removed by using the following device: Paste smoothly upon a slab of plate glass a one □ cm. piece of platinum foil perforated by a circular hole 1 mm. in radius and connected with one pole of a small battery. To the other pole of the battery attach by a fine flexible wire a steel needle, set in a wooden pen-holder. Required to hold the point of the needle at a fixed distance perpendicularly above the centre of the hole, and at command to tap the glass

within the hole. A telegraph sounder gives the signal if contact is made. If the hole in the platinum be of such a size that a considerable per cent. of trials falls within, and another considerable per cent. falls without it, then the number of hits within, divided by the whole number of trials is the empirical probability of hitting within that area. The radius x of the hole being known, the value of h can be determined from the Probability Integral. If desired, an x can be determined corresponding to a probability .683, that is, one can determine from the ascertained probability, and the known value of x, the x which must be used in order that 68.3% of the trials shall be successful.

If an electric counter were used (such as that of Ewald or the apparatus of Dr. E. C. Sanford, already mentioned), and if one hundred trials were made, the reading on the clock face would be the empirical probability (p) in per cent. of failing to hit within the hole; (100-p) would then equal the probability of succeeding. If the size of the hole were kept constant, the values of t in the Table of the Probability Integral $\frac{2}{\sqrt{\pi}}\int_0^t \varepsilon^{-t^2} dt$ would always be equal to $k\,h$, the value of k varying with the unit in which the radius of the hole is expressed. It would be very easy to have the Probability Integral Table printed upon a convenient card-board for immediate reference in the clinic; so that if the average value and variation of h in normal cases were determined, it would be a matter of a few moments to obtain this test of incipient ataxia.

As a clinical test, however, this method is open to one serious danger. Probably for the reason that the movement is an unusual one, the subject makes very awkward movements at first; and owing to the fact that the movement is not difficult to learn, he makes very rapid improvement. This is shown in the following record made by myself. Each number in the table represents the number of failures to hit within the hole, ten trials being made in each case. In one set of trials the wrist moved and the other joints were kept still; in a second set of trials the elbow alone was moved; and in a third set the shoulder alone was moved. The conditions were kept as nearly possible the same in successive trials. It is evident in each case that the comparatively small amount of practice has greatly reduced the number of failures.

TABLE XXIV.

Table showing number of failures in ten trials; 60 sets of ten trials each. Distance moved 1 cm. Trial every 2 seconds.

	Right.			Left.		
No. trials.	Shoulder.	Elbow.	Wrist.	Shoulder.	Elbow.	Wrist.
60	4	8	2	3	3	6
60	5	2	1	3	4	7
60	4	2	3	4	6	4
60	2	1	0	9	5	4
60	1	2	0	5	4	0
60	2	4	1	4	5	0
60	1	2	0	6	2	1
60	0	0	0	7	1	0
60	1	1	0	3	0	1
60	0	3	0	1	3	4
600	20	25	7	45	33	27

SCHOOL TESTS ON PRECISION.

Probing Movement.

With few exceptions the children tested by the foregoing methods were tested also for precision in the movement just described. Five trials were made with the right hand and five with the left. A board was held in position 6 mm. above the apparatus; the pen-holder was each time lifted until its upper end touched this board. It cannot be guaranteed that the pen will always be held in a perfect perpendicular, and accordingly the minimum distance to be moved, 6 mm., was sometimes slightly increased. If we assume what is quite certain, that in no case the pen was permitted to slant so much as thirty degrees, the maximum distance moved was always less than 8 mm.

The forearm was allowed to rest upon the table. The pen was directed mainly by the movement of the wrist, in a slight degree sometimes by movement of the elbow. Concrete directions by word and example, as in the writing move-

ment test, were given. No record was taken unless and until the child gave satisfactory evidence that the task required was understood. The results were classified according to the age and sex of the pupil and according to the hand used.

In the foregoing pages (192-193) the method of treating such results has been shown. It is only necessary here to say that in the manner described the probability of tapping within the hole without touching the platinum was determined for each set into which the results were classified. From this probability and the known value of x, the radius of the hole, were determined the values of h in each case, and the radii of circles within which in the several cases two thirds of the results would probably fall. Table XXV. gives these results and Chart V. shows graphically the same facts.

TABLE XXV.

Table showing in mm. the radii of circles within which 68.3% cases would fall.

AGE.		6	7	8	9	10	11	12	13	14	15	16
Boys,	Right	1.10	.97	.87	.80	.67	.65	.53	.58	.60	.42	.44
	Left	2.09	1.69	1.50	1.05	1.01	.90	.86	.87	.96	.79	.94
Girls,	Right	.91	1.01	.85	.86	.69	.69	.59	.61	.53	.49	.40
	Left	1.84	1.24	1.15	1.27	1.05	1.02	.83	.97	.74	.82	.77

EXTREME LIMITS OF VARIATIONS.

The amounts of decrease in the mean deviations between ages 6 and 16 are shown by the foregoing table to be: for boys' right .68 mm.; boys' left 1.30 mm.; girls' right .61 mm.; girls' left 1.10 mm.

These numbers are all larger than those in the corresponding table for the writing movement, page . That is, the mean deviations are very much larger and the decrease in the absolute size of the mean deviation between ages 6 and 16 is very much greater. The relatively great gain between ages 6 and 8 does not appear here so decisively as in the writing movement.

As in the writing movement, the left hand reduces the mean deviation much more than does the right hand.

Except at the age of six, where boys are inferior to girls, no decisive difference appears between the sexes.

Résumé of Results from Precision Experiments.

1. In normal individuals, the precision of voluntary movement is subject to much greater variation than is the maximum rate of movement. This test will probably distinguish pathological from normal deviations surely, only when the ataxia exists in a considerable degree, or when many tests are made. (P. 189.)

2. The absolute size of deviations from the movement attempted decreases much more rapidly in the two or three years following the age of six than later. This is particularly true in the case of the right hand. (P. 190.)

3. In right-handed persons, the right hand is superior to the left in precision, in about 80% of individual cases. Between ages 6 and 16, the deviations of left hand movements decrease by a greater absolute amount than do those of right hand movements. This is true for boys and for girls, in both directions of the familiar writing movement, and in the unfamiliar probing movement. (P. 191.)

4. The errors are, of course, greater with the unfamiliar probing movement under the conditions described than with the familiar writing movement. The decrease of the absolute size of the mean deviations is also greater. (P. 195.)

5. There is little mean difference in precision between boys and girls. These results indicate a slight superiority in favor of boys. (P. 192.)

Strength and Endurance.

Out of a large number of strength and endurance tests, only those will be reported at present which bear upon the question of bilateral development.(22)

Apparatus: The literature of dynamometry shows general dissatisfaction with the apparatus and methods which have been employed.[1]

It has been shown that varying mechanical factors, in the instrument, in the mode of gripping, or in the size and shape of the hand, co-operate with the quantity of force exerted to determine the record and therefore render the record doubtful. The following comparisons are made with the assumption that in the long run neither hand of the same individual would likely have any mechanical advantage over the other. A form of apparatus was devised essentially similar to that proposed by Hamilton(20), (that is, a mercury dynamometer, the mercury balanced by water, pressure being applied to a rubber bulb and transmitted by water to the mercury). Two

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[1]For a partial Bibliography of Dynamometry see Reference Handbook of Medical Sciences, II. 544. Cf. also Vierordt(21).

of these dynamometers were made and placed in the same frame about 40 cm. apart so that pressure could be applied to both bulbs at once, or to either at pleasure. In the following experiments, ample time was given to rest between trials, except that when the two hands were used successively, the succession was immediate. The order in which the hands were used was alternated in successive tests. The following table gives the number of times the preferred hand was superior in strength in the cases of seven (University) adults :

TABLE XXVI.

Subjects		J. A. B.	T. L. B.	G. S. H.	W. B.	F. B. D.	J. L.	E. C. S.	Total
No. of Trials		13	18	15	39	18	8	13	124
Preferred hand stronger	Hands used separately	5(6)	7(9)	0(2)	17(19)	7(9)	4(4)	6(7)	46
	Hands used together	5(7)	6(9)	11(13)	18(20)	5(9)	4(4)	5(6)	54
Total		10	13	11	35	12	8	11	100

The numbers in parentheses show the number of trials.
Number of cases, preferred hand stronger=100=80.6%.
" " " " " not as strong=18=14.5%.
" " " hands equal=6=5.%.

It is shown that the preferred hand exerts more strength than the other hand, whether the hands are used separately or together in about 80% of the cases. This is almost precisely the probability obtained from the school tests, that a right side joint will be faster in a given case than the corresponding left side joint. In this connection the claim of Féré([23]), that strength and rate vary together, and the suggestion that rate depends in part upon the intensity of innervation, will be recalled. Compare (p. 191) the probability of (R > L) in precision. Cf. Binet([30]).

The same experiments tabulated above were used also to determine whether more strength can be exerted by a hand working alone, or by the same hand when the other hand is working also. The results show that 5 of the 6 adult subjects were able to exert more strength with the hand working alone in most, but not in all cases.

In 60% of (112) cases, the hand working alone was stronger than when the other hand was working also.

In 27.5% of cases, the hand working alone was not so strong as when the other hand was working also.

In 12.5% of cases, the result was the same.

Table XXVII. shows the number of trials made by each subject, the mean height in cm., to which the column of mercury could be raised by each hand working alone, and by each hand working at the same time with the other.

Table XXVII.

Maximal Grip.

		Hands Separately		Hands Together	
		Right	Left	Right	Left
	No. of Trials	cm.	cm.	cm.	cm.
T. L. B.	(9)	115	112	108	106
E. C. S.	(8)	79	75	74	72
J. L.	(4)	80	66	76	61
F. B. D.	(9)	113	109	109	106
W. B.	(20)	99.6	96.6	99	95.6
J. A. B.	(8)	84.4	78	84.2	82

Note on Endurance Tests.

A few tests were made to determine the number of seconds an amount of force equal to about two thirds of the maximum force could be exerted. It came out very clearly that the right hand has greater endurance than the left; that the endurance of each hand is greater when working alone than when working at the same time with the other; and that the endurance of each hand is lessened if the other hand has been wearied by an endurance test. These results, however, were obtained only upon a single subject, and require verification.

Table XXVIII.

Subject W. B. Table Showing Results of Endurance Tests.

		Hands Working Separately		Both Hands	
	No. of Trials	Right Hand	Left Hand	Right	Left
First Set	8	36″	30″	26″	26″
Second Set	10	36.1″	30.5″		

Av. endurance	of	right	hand	when	left	was	wearied		32.6″
"	"	"	"	"	"	"	"	unwearied	33.7″
"	"	"	left	"	"	right	"	wearied	28.4″
"	"	"	"	"	"	"	"	unwearied	31.2″

Average superiority of right over left when right hand came before left: 10.″

Average superiority of right over left when right hand came after left: 1.2″

THEORETICAL.

In the foregoing pages I have given the immediate results of the experiments made, without theoretical comment or inference. Following are suggestions of probable conclusions from the facts:

I.

The maximum rate of voluntary rhythmically repeated movement is probably a critical test of voluntary control.

(a) While it does not appear that the rate of rhythmical "resulting movements" is identical with the rate of innervation, it can scarcely be doubted that the rate and intensity of innervation affect the rate of resulting movements. Von Kries says:

"Das wir in dem Rhythmus der Muskel Anschwellungen wirklich den Rhythmus der Inervation erhalten, das wird, wie ich glaube, kaum bezweifelt werden." This has been assumed by most studies of the rate of innervation. (24, p. 9. See also 25, p. 10.)

If this be true the decrease in the rate of the muscle rhythm (Griffiths, see p. 138) with fatigue is in all likelihood cause or part of the cause of the simultaneous decrease in the rate of the "resulting movements."

(b) The maximum rate of rhythmically repeated voluntary movement is probably a test of the power of voluntary arrest and reversal.

It was shown from experiments by Von Kries (p. 138), and confirmed (p. 150), that variations in the amplitude of movement within wide limits do not affect the rate. This paradoxical result,—which I have found to hold also for eye movements within the angle of usual movement,—is due to one or both of the following causes. For small distances, and especially for distances less than those usually passed over, the times of arrest, reversal, and of passing through the space nearest the points of reversal, may be slightly increased. In all cases the sum of these times must be so great in proportion to the time occupied in passing through the middle space that a slight increase of the latter is inappreciable.

That is to say, the rate of tapping is almost identical with the rate of voluntary arrest and reversal.

(c) The maximum rate of movement probably furnishes a test of the general condition of the central nervous system.

In this connection, Dresslar's(26) demonstration that mental excitement increases the rate, and that each day the rate probably varies with the tone of the central nervous system, is especially significant.

(d) The maximum rate of movement probably furnishes a test of the condition of the nerve centers by which the muscles involved in the movement are controlled.

If the conclusion of Mosso(27), Maggiora(28) and Lombard (29) be correct, that the working of a joint produces central fatigue, it is probable that the cases in which working a joint was followed by a lowering of its rate are to be explained in part, at least, as due to the effect of central fatigue. The fact that the rate of a joint is lowered by local fatigue while the rate of other joints remains unaffected, indicates no finer physiological differentiation in the central nervous system than the fact that we are able voluntarily to move one joint while the adjacent joints remain still. It would strongly confirm this view if it should appear that fatigue through one joint affects the rate of the corresponding joint on the other side.

II.

The History of the period from 12 to 16 in girls (see pages 161 and 174) and from 13 to 16 in boys, exhibiting in turn acceleration, decline and recovery of rate ability, presents what is, at any rate, a suggestive analogy to the course of ordinary over-tension, fatigue and recovery of the nerve centers. It would seem something more than a reasonable surmise that the general acceleration of the rate in girls from 12 to 13, and in boys from 13 to 14, is an expression of high tension in the nerve centers in many individuals at those ages; that the decline following is an expression of nervous fatigue consequent upon the functional changes at those periods; and that the re-acceleration is a sign of recovery from that fatigue. It is significant that (page 174) the antecedent acceleration and the decline are more extreme in girls than in boys, and that the girls recover more slowly. It seems not unlikely that these facts may prove of hygienic significance.[1]

[1] Note the result of Bowditch (8, 10 and 22, Reports State Board of Health of Mass.), of Peckham (6th Report State Board of Health of Wisconsin), and of others reported by them showing that boys exceed girls in weight and height at all ages from 5 to 18, except from about 12½ to 14½. Dr. Gerald M. West tells me that the measurements of Worcester school children by Dr. Franz Boas and himself show the same result. See also Prof. Bowditch's explanation by the theory of antagonism between growth and reproduction. (Op. cit. 8, 283.)

III.

The fact that the hand is at first inferior in rate ability to the arm (pp. 169-172) is perhaps explained, and its genetic significance is emphasized, by the observation made upon nearly, if not quite, every child of five and six, that the clasping tendency is still very strong. This is shown by the evident $\overset{\prime}{——}\ – \mid \overset{\prime}{——}\ – \mid \overset{\prime}{——}\ – \mid$ rhythm of up and down strokes or by testing the force of downward pressure.

IV.—Résumé of Results Touching Bilaterality.

I. In the right-handed subjects, the right hand and arm are superior to the left in strength, rate and precision in a majority of trials. In few subjects is the right hand superior in every trial. The probability from these researches that in a single trial taken at random the right hand will exceed the left in strength, rate or precision is about 75% to 80%, the probability that the right hand will not be inferior to the left in such a trial is about 85% to 90%.

2. The effects of effort through either upper extremity are probably shared in some degree by both. (a) Between the ages 6 and 16 the right hand and arm very little, if at all, outgrow the left in rate ability. Since it is certain that the right side joints have vastly more use, either the growth of rate ability has not been determined by use, or the effects of use on the right side have been shared by the corresponding joints on the left side. The fact that Dresslar found no effect from practice is to be taken in connection with the fact that his records show very little effect from practice upon the right hand. The preliminary practice, while perfecting apparatus, had, he thought, trained his hand about to its maximum before records were taken.

(b) Between the ages 6 and 16 the mean deviation from the movement intended is reduced by a greater absolute amount by the left hand than by the right. We are not justified in assuming that reduction in the absolute size of the mean deviation by a certain amount means the same degree of gain in voluntary control, whatever the mean deviation from which the reduction is made. It is certainly easier to reduce a mean deviation x to $\frac{x}{2}$ than to reduce the latter to zero. It would be a rash conclusion that there is a gain in precision by the right hand between 6 and 8 equal to its gain between 8 and 16, because the reduction in the absolute size of the mean deviation in the first period is about equal to that in the second. The fact that between the ages 6 and 16 the

reduction in the absolute size of the mean deviation, in the case of the unfamiliar probing movement, is about six times as great as that made in the case of a movement constantly practiced, can certainly not mean that there is actually a greater gain where there is infinitely less use.

The fact that decrease in the size of the mean deviations from the movement intended is greater with the left hand than with the right, with the right when it is little developed, in the less practiced movement, and that as the deviations become less, their reduction becomes slower,—must rather be held to indicate that great absolute reductions are characteristic of relatively low development; and that slow and steady reductions are characteristic of comparative escape from physiological ataxia. It is, nevertheless, certain that the right hand does not outgrow the left; and the fact that, at 15 and 16 years of age, the probability of (R $>$ L) is less than at 12, 9 or 6 years of age, seems to indicate that the left has actually gained upon the right. At all events the fact that the left hand should make such relative improvement both in ability to carry out an unpracticed (probing) movement, and in ability to carry out a movement in which the right hand has had all the practice, tends to confirm the probability of bilateral effects of practice. It is, of course, not to be forgotten that the practice in this case is largely mental. (See Stumpf([31]), Cattell & Fullerton([32]), Camerer([33]), Fechner ([34]).

(c) The amount of force which can be exerted through one hand and the time during which it can be exerted depend upon whether at the same time or just preceding, force has been exerted through the other hand.

(d) The maximum rate of a joint is *possibly* affected by the exertion of the corresponding joint on the other side in the time just preceding.

4. Corresponding joints have generally the same periods of acceleration and retardation of growth. But there is nearly always a considerable bilateral asymmetry of development; and the asymmetry is generally the greater as the growth is more rapid. The fact that the boys' right arm grows faster in rate-ability than that of girls, and grows more asymmetrically as compared with the left, that the hands outgrow the arms and show more bilateral asymmetry, and that periods of rapid growth are generally periods of increasing bilateral asymmetry, show that bilateral asymmetry is not to be regarded as abnormal, but rather as in some degree an attendant and sign of growth.

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Note.—I heartily acknowledge my indebtedness to all the members of the Psychological Faculty of Clark University for advice and assistance in this and other work in the University. Above all I am under obligation to Pres. G. Stanley Hall for directing me to and in the following research.

Specific credits are given in the text.

Explanation of Rate Chart I.

1. Abscissa=time. Years noted at bottom of chart.
2. Ordinate=rate. Seconds indicated on margin.
   The base line is assumed to represent 4 taps per second.
   All points are then fixed with reference to that line.
3. The solid lines connect the mean-rate points of successive years.
4. The inside dotted lines represent the limits of mean variation of the means.
5. The outside dotted lines represent the limits of mean individual variation.
6. Boys' charts on the left of the middle line; girls' on the right.

Explanation of (R-L) Chart II.

1. Abscissa=time. Years noted at bottom.
2. Ordinate=values of (R-L). Base line: (R-L)=o. R-L=1 indicated on margin.

———————— = Finger.
. . . . = Wrist.
—o—o—o—o— = Elbow.
++++++++++++++++ = Shoulder.

Boys to the left; girls to the right of middle line.

Explanation of Bilateral Asymmetry Chart III.

Abscissa=time. Years noted at bottom of chart.

Ordinate=values of $\sqrt{\mu_R^2+\mu_L^2}$=function of mean individual variations of R and of L, and of μ_B=mean variation of individual (r-l) values from this mean. Value of ordinate=1 tap per sec. indicated on margin.

———————— = Line connection values of $\sqrt{\mu_R^2+\mu_L^2}$.
. . . . = " " " " μ_B.
Base line = " whose ordinate is zero.

Boys to the left; girls to the right of middle line.

Explanation of Precision: Probing Movement Chart IV.

1. Abscissa=time. Years noted at bottom of chart.
2. Ordinate=values of the radii of circles within which 68.3% trials would fall. Values of ordinates, 1 mm. and 2 mm. indicated on margin.

The upper lines represent the records for the left hand.
The lower lines " " " " " right hand.
Boys to the left; girls to the right of middle line.

Chart II. Rate: (R.–L.).

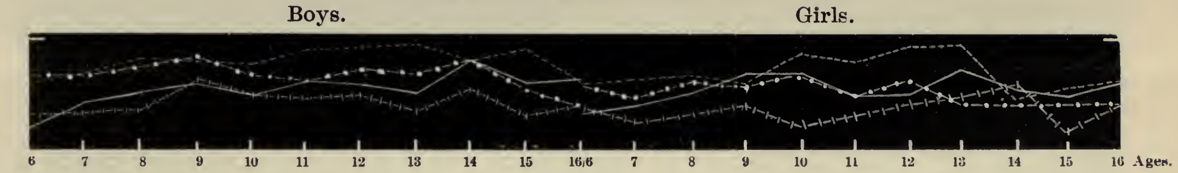


Chart IV. Precision: Probing Movement.

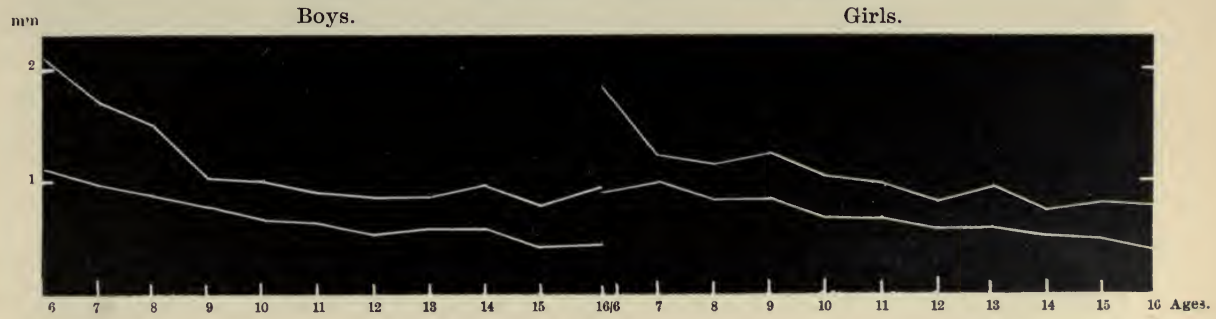
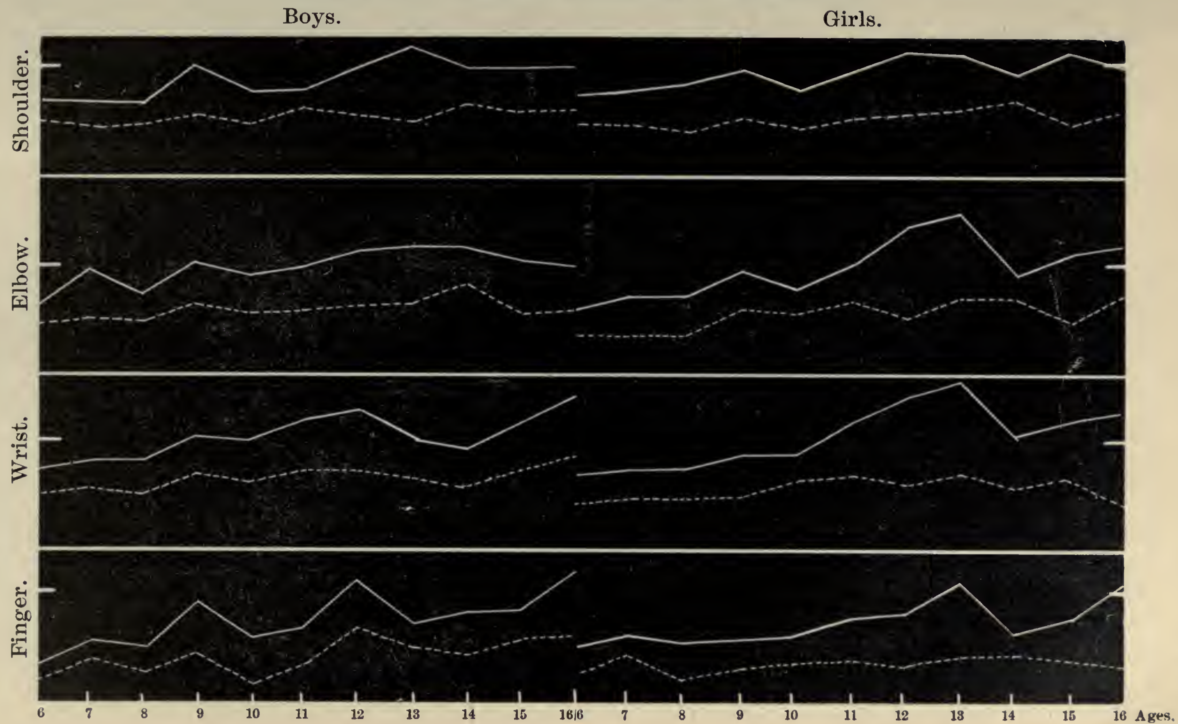


CHART III. RATE: BILATERAL ASYMMETRY.



THE TRAINING OF ANIMALS.

By James E. Le Rossignol, Ph. D.

A.—Dogs.

Professor Brewer of the Sheffield Scientific School has kindly loaned for the benefit of the writer the choicest of his invaluable collection of books on dogs. At the suggestion of Dr. Hall, the following notes have been prepared on these and several other works, and may be of interest to students of animal psychology.

Xenophon's Kynegetikus is the oldest known work on the subject. Among the Greeks different races of dogs were used for different kinds of game. For hares, Castorians and fox-dogs were used; for stag-hunting, Indian hounds, and for the wild boar, Indian, Cretan, Locrian and Laconian hounds. Chief attention is given to the coursing of hares. The dogs were not expected to catch the hare, but to drive it into nets which were set at certain places. The best dogs are those with a light head and blunt muzzle, prominent black shining eyes, broad forehead, long, flexible, round neck, broad chest, straight elbows. They must be strong, well-proportioned, swift of foot, and above all they must be keen-scented. Training must begin with young dogs. They should be kept in good health by nourishing food and plenty of exercise. The trainer himself should feed them, that they may become attached to him. When a pack is taken out to hunt, the older dogs are loosed first and then the younger are permitted to follow the scent with the others. They must usually be fed near the nets, that they may not wander too far from them, and they must be taught to drive the hare in that direction. If the scent be lost they must circulate until they find it again. As a reward the young dogs may be allowed to worry the hare. They are not to contract the habit of following foxes or any other animal than the hare. They should be taken out to hunt every two or three days, that they may become accustomed to the chase.

Arrian's Kynegetikus adds very little to what had been already said by Xenophon beyond recommending a few new races of hounds.

Of Englishe Dogges; a short treatise written in Latine by Johannes Caius, *and newly done into Englishe by* Abraham Fleming. London, 1576, pp. 44.

A curious book, in which the author, John Kay or Caius, gives his friend, Conrad Gesner, a description of the principal races of English dogs. It is interesting to notice that nearly all the chief races of the present time are mentioned by him.

Praktische Abhandlung von dem Leithund, CARL VON HEPPE. Angsburg, 1751, pp. 499.

No doubt at the time a valuable manual for huntsmen on the use of the led-hound. The hound was kept always in the leash and was used in tracking wild animals to their haunts. When once the game had been found a hunt was organized in the regular manner. The chief requisite for a led-hound is a keen scent. Training begins when the dog is but a few weeks old, and from that time he is in every way possible accustomed to the scent of the game he is intended to track. If it be deer, pieces of deer's flesh or skin are given him to smell and tear. When a little older he should be taken in the early morning to a place where the deer have lain during the night and encouraged to smell about and follow the scent as far as possible. Again, in a meadow a piece of warm bread or deer's flesh is trailed along the ground and the dog induced to follow the scent and allowed to eat what he finds. If he tries to find the scent in the wind his nose must be pressed down to the earth until he learns to sniff about on the ground. If he fails to follow the scent he is beaten, but if he does well he is caressed and rewarded. A tendency to chase birds or rabbits must at once be checked. He must be taught to love his master and always to obey his commands. He must be gradually accustomed to follow a weak scent by being kept at work until a late hour every day, when the sun has partially removed the scent. Restraining with the leash counteracts too great eagerness and prevents the dog from running over the scent, while words of encouragement and rewards tend to increase his ardor.

Ansführliche Geschichte der Hunde, ANON. Leipzig, 1781, pp. 374.

The author, who styles himself Kynophilus Actacon, begins his work with a chapter on the psychology of the dog. Dogs, as well as men, are either of choleric, melancholic, sanguine or mixed temperament. The chief faculties of the dog's *moral nature* are genius, instinct and memory. Genius is the power of quickly learning and comprehending anything. Dogs also possess the power of forming ideas, and exhibit attention and curiosity. The will is determined by innate tendencies or impulses, or in other words, by instinct, and forms the most important part of the dog's *moral nature.* Among the various impulses are desire for food, procreative impulses, impulse towards self-preservation, desire for amusement, exercise, fresh air, and for society. By means of these impulses the dog is impelled to eat, to mate, to hunt and fight and flee danger, to play and run and to associate with his kind and with men. It is by taking advantage of these impulses that man is able to train the dog in so many different ways. Dogs are trained by means of a judicious use of rewards and punishments, by the association of pleasant feelings with certain actions and painful feelings with others. The dog's own impulse towards self-preservation leads him to obedience, and when habits are formed rewards and punishments are no longer necessary. The training of dogs must be varied according to the ends to be attained, but the principles to be followed remain the same.

Der Hund, seine verschiedene Zuchten und Varietäten, DR. FRIEDRICH LUDWIG WALTHER. Giessen, 1817, pp. 96.

The chief characteristics which render the dog so useful to man are the keenness of his senses, his quickness of perception, his teachability, power of memory, faithfulness and obedience.

The General Character of the Dog Illustrated by Anecdotes, JOSEPH TAYLOR. London, 1821, pp. 150.

As with most anecdotes on canine sagacity, the lack of exactness and complete explanation of circumstances render it impossible to draw any safe conclusions from the instances here related.

Kunopædia; a Practical Essay on Breaking or Training the English Spaniel or Pointer, WILLIAM DOBSON. London, 1814, pp. 235.

The dog is to be gradually accustomed to obey certain commands by voice or gesture. Before being taken to the field the dog must learn to obey the commands, "come here" and "down." Obedience is rewarded by caressing, while the contrary is punished with the whip. After whipping, the master must make friends with the dog before letting him go, and he must never be chased, that he may not learn he is swifter than his master. The method used in the field is much the same as that given by Col. Hutchinson, except that more severe punishment is recommended. The great principle to be followed is that of complete obedience.

Canine Pathology, DELABERRE BLAINE. Lond. 1832, pp. 310.

This was the first scientific work on the subject and the author has been called the father of canine pathology.

The Dog, WILLIAM YOUATT. Philadelphia 1857, pp. 392.

A more complete work than the former and long the standard work on the subject.

The author thinks that in regard to intellectual powers the difference between the dog and man is in degree and not in kind. The brain of a man weighs about $\frac{1}{30}$ of the weight of his body, that of the Newfoundland dog $\frac{1}{60}$, while that of the bull dog weighs only $\frac{1}{360}$ of his entire weight. So great is the influence of heredity that instances have been known of dogs which at the age of a few months were self-broke and required no special training for the chase. If these induced habits, however, are not cultivated, they are soon lost. The pathology of the dog closely resembles that of man, both in the nature of the diseases and the mode of treatment. Between the mental conditions of the dog and of man in disease there would seem to be a striking parallel, especially in the case of hydrophobia.

The practitioner must understand the nature of the dog. He must be firm and courageous as well as kind and gentle in order to make the dog sensible of his power and of his good intentions. A sick dog often seems to realize that the doctor is endeavoring to relieve him, and will often lick the hand that for the moment causes him pain.

Vollständige Unterricht, alle Arten Hunde abzurichten, CHRISTIAN FRIEDR. GOTTL. THON. Ilmenau, 1832, pp. 296.

The trainer must be careful to select a dog of the proper race, size and temperament for the particular kind of hunting he is to learn. Still, the natural temperament of the dog may be greatly modified by careful training. The trainer must not only know the nature of the dog, but must exercise great care and patience. The dog must learn to know and love his master and the master must treat him kindly and never punish him too severely. Punishment should take place immediately on the commission of a fault, but should be in general by words rather than by whipping.

Training must proceed step by step from the simple to the more complex. Too much ought not to be attempted at once. Frequent repetition is necessary. As a reward for good behavior the lesson may be shortened, or it may be lengthened if the dog be lazy and disobedient. The lesson should take place when the dog is somewhat hungry, for then his faculties are on the alert.

Instruction must always be accompanied by words and the same words must always be used with the same meaning.

Dogs: Their Sagacity, Instinct and Uses, GEORGE FREDERIC PARDON. Lond. 1859, pp. 306.

The author defines instinct as "a faculty given to the lower animals instead of reason, a faculty that teaches them all they require to know, a governing principle that impels them to do all that is necessary for the preservation of their lives and the continuance of their species, and it operates without the aid of instruction or experience."

House Dogs and Sporting Dogs, JOHN MEYRICK, Lond. 1861, pp. 250.

A handbook intended for the use of dog-breakers. Methods of training must be adapted to the disposition of each particular dog. Some dogs are rendered vicious by whipping, while others require it to subdue their obstinacy.

The master should train his own dog. If training be carried on gradually and if each lesson be thoroughly learned before proceeding to the next, great progress may often be made in a short time. A retriever, for instance, is first taught to fetch and carry, then to follow the scent of a piece of meat that has been trailed along the ground, then to track a lame rabbit through long grass and to fetch it without injury, and finally to retrieve game in a similar manner. Many amusing tricks may easily be taught by taking advantage of some peculiar habit of a dog. If a dog be in the habit of standing on its hind legs for food, it can easily be taught to dance by holding food above its head. When two or three such tricks are learned, others are acquired much more rapidly.

Breeding, Training, Management, Diseases, etc., of Dogs, FRANCIS BUTLER. Brooklyn, 1879, pp. 391.

Almost every dog, the author thinks, is capable of a high degree of training, although some breeds, owing to heredity, seem more apt to receive instruction than others. Training should begin as early as possible, for it is much easier to teach right habits than to break off wrong ones. The dog should learn to obey from a desire to please his master rather than from a fear of punishment. Only one person, preferably the owner, should teach the dog, and a consistent system should be used, that the dog may not become confused at the different commands and methods. The same sign or word should always have the same meaning. The trainer should always show pleasure when an act is rightly performed and displeasure when a mistake is made. Advantage should be taken of the instinctive actions of dogs. Thus a puppy seems instinctively to run after things and carry them about, and a little training will teach him to do this at the word of command. Similarly, some dogs seem instinctively to take to the water, while others have an instinctive passion for hunting. Dogs have retentive memories and associations between commands and actions are easily formed, and thus a dog's *vocabulary* may gradually be enlarged.

Dog-Breaking, GENERAL W. N. HUTCHINSON. Lond. 1876, 6th Ed., pp. 348.

This book treats chiefly of the training of pointers and setters and is probably the best work on the subject.

The breaker should possess a thorough knowledge of dog natures. He should have perfect command of his temper and should use no severity. He should be consistent, that is, he should reward the dog for acting rightly, even when he has missed the game, and should not fail to punish a mistake even when the game has been bagged. He must act with reflection by considering what meaning the dog attaches to every sign and word and always using the same words with the same meaning.

Training should begin with young dogs at home. They must be taught to pay attention to the whistle and to obey certain words and signs. Of these, the word "toho," accompanied by raising the right arm, is the command to stand still; the word "on," with a forward under swing of the right arm, means that the dog is to advance in the direction indicated by the hand. "Drop," is the order to crouch. The orders should gradually be given in a low voice until finally the gesture alone is used. The dog must be accustomed to watch his master for commands. When these and several other commands have been well learned, the dog is taken to the field. He is expected to discover the bird by the taint in the air and not from the ground. Therefore he should enter the field at the leeward side. The trainer may use a check cord of from ten to twenty yards in length. On entering the field he sends the dog from him towards the wind by a wave of the hand or the word "on." The trainer makes the dog beat the field from side to side while he advances up the middle. When the dog has gone far enough in one direction, he is called back by a wave of the left hand or by a whistle, or, if necessary, he is pulled around by the check cord. As he passes, the trainer encourages him on in the opposite direction. When, by watching him, the trainer observes signs of his having scented a bird, he raises his right hand. If the dog does not stop the word "Toho" is given, and if he still refuses to stop the cord may be used and the dog pulled back to the place where he should first have pointed and there punished with constant repetition of word and sign until he obeys. If now the bird rises and the trainer fires, the dog must crouch at the word "down charge" and must remain in that position until his master has reloaded and given the word "seek dead," when the dog must find the game, yet is not allowed to take it in his mouth.

After a few weeks' practice with a careful trainer a dog will begin to acquire the proper habits of hunting. Much depends on the preliminary lessons, which should be thoroughly learned before more difficult feats are attempted.

The Dog; by Dinks, Mayhew and Hutchinson, FRANK FORESTER. New York, 1873, pp. 655.

A compilation of Dinks' book on the general management of dogs, Mayhew's Canine Pathology and Hutchinson's Dog-Breaking. Mayhew lays especial stress on the fact that the dog is essentially a nervous animal and that regard must be had to this both in the training of dogs and in the treatment of their diseases.

Die Hundezucht im Lichte der Darwinschen Theorie, GUSTAV LUNZE. Berlin, 1877, pp. 231.

Artificial selection and training by man have been the chief causes of the great variations now existing between the different

canine races. If left to themselves the different races would probably revert to a few types resembling some variety of wild dog.

Der Vorsteh-Hund, FRIEDR. OSWALD. Leipzig, 1873, pp. 290.

An exhaustive treatise on the training of the setter.

The American Kennel and Sporting Field, ARNOLD BURGES. New York, 1876, pp. 201.

The essential qualities of a good hunting dog, says the author, are nose, staunchness, pace, endurance, intelligence and high-breeding. These should all be present in a proportionate and well-balanced degree. All of these characteristics are hereditary. Also the particular methods of hunting are hereditary. The progeny of a pointer shows a natural tendency to point game. Highly bred dogs also as a rule show greater intelligence than others. Therefore, in breeding, both parents should be of good stock and well accustomed to hunting.

It is best for a sportsman to break his own dog, for the dog becomes accustomed to the sound of his voice and his general manner and becomes attached to his first master as to no other. Also dogs always hunt best the birds they are broken on. In both these cases first impressions are the strongest and most enduring.

No set of rules will apply to all dogs, and there is a wide field for the exercise of common sense.

The Dog; with Simple Directions for his Treatment, "IDSTONE." London, 1872, pp. 252.

This is a standard work on dogs, chiefly descriptive of the different varieties. The English fox-hound is adduced as a remarkable example of the excellency to be obtained by careful breeding.

The Dogs of the British Islands, "STONEHENGE." London, 1892, pp. 279.

The editor, one of the greatest authorities on dogs, has collected from various reliable sources accounts of all the principal canine races.

Le Chien; d'après les Ouvrages de STONEHENGE, YOUATT, MAYHEW, BOULEY, HAMILTON SMITH, &c. Paris, 1876, pp. 330.

A valuable and condensed compilation from the works of the standard authorities on dogs.

The Dogs of Great Britain, America and other Countries. New York, 1879, pp. 366.

Compiled from two works by Stonehenge and some chapters by American writers.

The description of the German Dachshund is suggestive of the very different modes of treatment which must be used with different dogs. "Dachshunds are head-strong and difficult to keep under command; and, as they are at the same time very sensitive to chastisement, it is next to impossible to force them to do anything against their will. Many good badger dogs have been made cowards for life by one severe whipping. They must be taken as they are, with all their faults as well as their virtues."

The Practical Kennel Guide, GORDON STABLES. London, Paris, New York, pp. 200.

Dogs; their Origin and Varieties, H. D. RICHARDSON. New York, pp. 127.

Dogs; their Points, Whims, Instincts and Peculiarities, HENRY WEBB. London, pp. 347.

The author thinks the bad reputation of the bulldog to be largely the result of his being in general chained up and trained for fighting, and thinks him capable of being taught anything as well as any other breed. Out of eight bulldogs owned by a certain gentleman, six learned to retrieve by land or water, and only one was at all bad-tempered.

Dogs and their Ways, REV. CHAS. WILLIAMS. London, pp. 376.

Especial attention is drawn to heredity of acquired habits. A puppy of a dog who had been taught to beg, at the age of seven or eight months spontaneously took to begging for everything it wanted. Other curious illustrations are given of similar heredity.

Natural History of Dogs, Vols. 18 and 19 of the Naturalists' Library, by COL. CHAS. HAMILTON SMITH, pp. 566.

Chiefly a description with plates of the various canine races, including the wolf and jackal.

A Guide to Dog-Training. New York, pp. 105.
Dog and Gun, JOHNSON J. HOOPER. New York, pp. 105.
Des Hundefreund par Excellence, MATTHIAS ALISTA. Wien. pp. 73.
Die Dressur des Hundes, ED. ZBORZILL. Berlin, pp. 202.

The author is a professional dog-trainer. Many young dogs, especially those of hunting breeds, very early show a tendency to fetch and carry, and every young dog likes to run about with things thrown to him. When a ball or glove is thrown to him he must be induced to bring it to his master at the word "fetch," and lay it down at his feet. If he will not let it go it must be gently disengaged from his teeth and placed on the ground at the proper place. Then a piece of bread or meat should be given to the dog, and he should also be patted and praised. On a second trial he will be more likely to lay the glove at his master's feet, and after a few lessons he will always do it so. Little by little the lesson may be varied and more required, until the dog will seek anything at the word "seek," or will learn to run errands with a basket.

There should be no whipping at first, but in every case something to eat should be given the dog when he has done anything properly, and he will soon learn to take pleasure in trickperforming.

The most difficult tricks are only combinations of very simple performances. The so-called counting and spelling by dogs is taught by mechanical devices aided by words and signs which the dog learns to associate with the action required.

A dog is taught to walk or dance on his hind legs by placing him in the position and aiding him with a stick, or it may be taught by holding a piece of meat above his head. He may be taught to turn round on the floor by tying a bit of meat to his tail. He will shut the door with his fore paws if a piece of meat be so held that he must climb against the door to reach it. In every case words of command are used and in time the dog will obey the word without the device that was necessary at first.

Researches into the History of the British Dog, GEORGE R. JESSE. London, 1866, 2 vols. pp. 385-424.

A most valuable work from a literary and historical point of view.

The History of the Mastiff, M. B. WYNN. Melton Mowbray, 1886, pp. 222.

The author is of the opinion that the English mastiff has from the earliest times existed in Britain, and is the ancestor of the bulldog.

Histoire du Chien chez tous les Peuples du Monde, ELZÉAR BLAZE. Paris, 1843, pp. 460.

A valuable work, dealing chiefly with the question of canine sagacity. The characters of dogs show much greater variety than those of wolves and foxes, because of long association with man. The dogs of civilized nations show much higher development of character than those of savages. The disposition of the male is also very different from that of the female. The character of a dog depends on the education he has received, on the kind of people about him and especially on the conformation of his skull. Education can do much to change the natural disposition of a dog.

A dog is essentially a creature of habit. What it is in the habit of doing that it does with pleasure.

There are two factors in a dog's mental development, his own experience and his education. A dog with a wide experience like a collie is more intelligent than one whose experience is confined to his kennel and his master's yard. A large circle of ideas implies greater intelligence. One has but to watch the behavior of an old and a young dog while hunting to see how much a dog will learn by experience.

The author says with regard to inherited instincts: "The dog is instinctively a hunter. We have adopted certain varieties to hunt for us, and they have become more apt than other dogs, but it is none the less true that all dogs are hunters by nature."

Histoire physiologique et anecdotique des chiens, BÉNÉDICT HENRY RÉVOIL. Paris, 1867, pp. 394.

A very complete work, descriptive of all the principal canine races. The author believes the dog to have been domesticated by man from the very earliest periods. Some of the oldest Chinese, Indian, Assyrian and Egyptian writings mention the dog as a domestic animal. The hunting instincts of the wild dog still survive in the hound, and the great ferocity often exhibited in the chase would seem a temporary recurrence to savage life and habits.

Le Chien; Histoire Naturelle, avec un Atlas de 67 Planches et 127 Figures, EUG. GAZOT. Paris, 1867, pp. 546.

A more scientific work than the last. The origin of the dog is unknown, although the fact that dogs when crossed with wolves and jackals produce a fertile offspring, seems to point to a common origin of all three families.

The influence of heredity and education on the physical and mental development of the dog is very great. Judicious crossing tends to the development of strength and intelligence, and too much in-breeding acts in the opposite direction. There is in dog a continual tendency to degenerate, which comes into operation whenever the care of man is removed, and a "reversion to type" is ever imminent.

The author refers to Darwin's observation on the training of sheep dogs in Montevideo. When quite young the dog is removed from its mother and placed with the flock, where its entire life is to be spent. Frequently immense flocks of sheep are left at a distance of ten kilometres from any habitation, entirely in the care of these dogs. The affection they show for the sheep is remarkable.

The great value of watch-dogs depends chiefly on two faculties, their acute sense of hearing and the restlessness they feel when they hear the least strange sound. By properly directing these faculties a good watch-dog is produced. In the training of dogs habits are to be acquired through a repetition of experience. Mental and physical qualities must be developed together. The natural instincts of the dog form the basis of training. Commands are given by voice and hand and the dog is accustomed to associate certain actions with certain signs, obedience with reward, and desobedience with punishment. The trainer should often himself do what he wishes the dog to perform. Thus when a shepherd trains his dog to lie down at the word "down," he lies down himself and makes the dog do the same.

Patience and time are always necessary, but the dog's natural instinct and intelligence aid in the speedy formation of habits, and with succeeding generations, owing to the power of heredity, the work is less difficult.

The Dog in Health and Disease, WESLEY MILLS. New York, 1892, pp. 407.

A complete and valuable work chiefly in regard to canine pathology, but also containing a chapter on breeding and training. Dr. Mills says: "A puppy, full of life, tends to do exactly as his impulses urge him, till the highest motive power, a desire to please his master, is substituted." Training, therefore, is a question of the direction of impulses by the formation of pleasant and painful associations, leading finally to the establishment of the habits desired. With regard to the methods to be employed the author is in accord with the best authorities.

ON THE JUDGMENT OF ANGLES AND POSITIONS OF LINES.

A.—On the Judgment of Angles.

By Joseph Jastrow, Ph. D.

With the assistance of Geo. W. Moorehouse, Fellow in Psychology.

The nature and extent of our errors in estimating and reproducing angles are the subject of our present inquiry. The point acquires a special interest from the fact that a number of writers have based their explanations of important optical illusions upon the view (apparently not tested by experiment) that acute angles are underestimated and obtuse angles overestimated. Such an investigation naturally begins with a definite mode of judgment or reproduction under definite circumstances, and is then to be supplemented by an investigation of the extent to which the results obtained are due to peculiarities of the method employed. We selected the angles 15°, 30°, 45°, 60°, 75°, 90°, 105°, 120°, 135°, 150°, 165° as our standard angles and drew these upon circular pieces of cardboard (3 in. in diameter); the lines themselves were 30 mm. long and were so placed that one line was always horizontal. The papers on which the angles were to be reproduced by the subject were placed on a table and the drawings made in the normal writing position. For convenience all acute angles were formed on the left hand side and obtuse ones on the right, so that the drawing might be uniformly from left to right. The subject viewed the angle as long as was needed to fix it in his mind (from 5 to 15 seconds) and immediately thereupon, from his memory of the angle, drew another as nearly as possible equal to the first. He did this by adding a line to a horizontal line of 30 mm. which was given him (always in the same position) upon the squares of paper upon which he drew. The drawing was done with a hard, well-pointed lead-pencil. Each of the standard angles occurred twice in a set of 22 angles, the order of the angles being determined by chance. The results of the measurements of 62 such sets, or 124 reproductions of each angle by 13 subjects in all, are shown in the following table :

Standard Angle	15°	30°	45°	60°	75°	90°	105°	120°	135°	150°	165°
Average Reproduction	18°03′	30°27′	42°09′	56°25′	69°44′	90°06′	113°34′	123°55′	136°29′	150°33′	163°49′
Error (from corrected standard)[1]	+2°23′	-0°56′	-2°16′	-5°13′	-6°46′	+0°06′	+7°54′	+3°22′	+0°19′	-1°05′	-2°41′

[1] For details of measurement and preparation of tables and curves, consult the note at the end.

It will be seen that the overestimations and underestimations can hardly be said to follow any simply formulatable law, such as the underestimation of acute and the exaggeration of obtuse angles; their full significance appears only in the curve as given below (Fig. 1); in this curve the differences between the actual and the re-

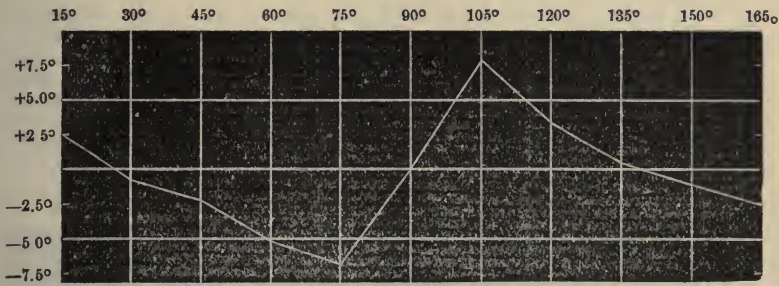


Fig. 1. Errors, in degrees, in reproducing angles by memory: Average of 13 subjects.

produced angles are plotted to the scale of one of the divisions to five degrees of error. This is our general result and its salient characteristics are, (*a*) the exaggeration of the angle at 15°, which passes into a gradually increasing underestimation up to 75°; (*b*) the correct reproduction of right angles; (*c*) the maximum exaggeration of the angle of 105°, which is followed by a decreased exaggeration, passing into an underestimation of very obtuse angles.

The next important inquiry is naturally how far the result is typical, and how far accidental; how far the result of the combination of different curves and how far the individual records agree with the general result.

By each of the thirteen individuals the angle of 15° is exaggerated; in all of the thirteen cases there is a falling off towards the next point 30°, the angle being about as frequently slightly overestimated as slightly underestimated; in all but two cases there is a fairly regular increase of the underestimation, reaching a maximum at 75°; in all cases the right angle is nearly correctly reproduced, the error being as often in one direction as in the opposite; in all cases the curve then sharply rises, reaching the maximum of exaggeration at 105°, and from there in eleven cases there is a more or less regular decline; the curve at the last point 165° falling below the line. Again, we may calculate the average deviation of the thirteen results from their mean; this for the eleven angles is 2° 31′, and of the 143 records (13 subjects for 11 angles) 87 show a deviation less than this average. Regarding the variation for the different angles it is least for 90°, the other angles following in this order, 150°, 165°, 15°, 30°, 135°, 75°, 45°, 120°, 105°, 60°.

Comparing the general outlines of the individual curves with the average curve, we find that in ten of the thirteen individuals, the correspondence is obvious and in most of these, striking; in one case the curve presents quite a different appearance, and in two other cases the differences are considerable. This diverging curve, however, is that of a professor of engineering, who has considerable experience in the estimation of angles; he draws angles of 30°, 90°, 135° and 165° very accurately, overestimates angles of 15°, 60°, 105° and 150° considerably, and underestimates the angles of 45°, 75° and

120°, thus presenting a zig-zag curve. The other two subjects came prepared with some practice in drawing and exhibited peculiarities in the mode of estimating the angles. In general there is thus a very striking similarity between the individual and the general result, so that the curve may be regarded as fairly typical for the average person.

The suggestion is close at hand that this result may be influenced by the mode of reproduction ; to test this, three of the subjects reproduced the angles, not from memory but with the standard angle constantly visible for comparison. The resulting curve is quite similar in the two cases, the essential difference being that the entire curve is closer to the true line of no error, i. e., the error is smaller. Fig. 2 shows the average result of the three subjects for each mode of reproduction.

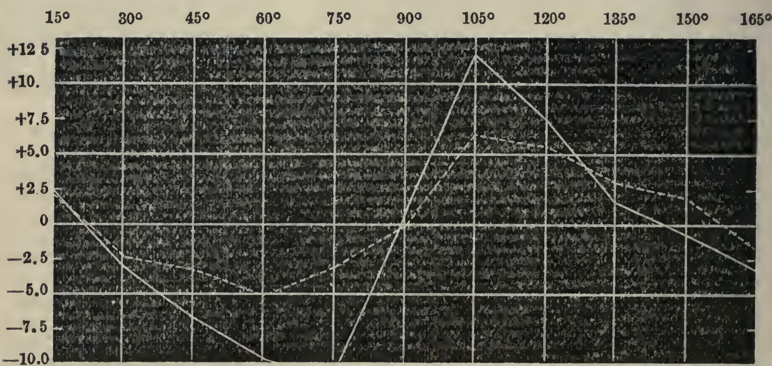


FIG. 2.

......Errors in reproducing angles with both angles visible. Average of three subjects.
———Errors in reproducing angles by memory. Average of same three subjects.

It is likewise interesting to determine the regularity and accuracy of these methods of reproducing angles ; a sufficient test of this is the average deviation of the results from their mean. This is very troublesome to calculate and we have contented ourselves with doing it for two individuals. In reproducing angles by memory the average deviation of the one subject (fourteen judgments of eleven angles) was 2° 50′, of the other 3° 05′; in reproducing angles with the standard angle in sight 1° 33′ for the one and 2° 08′ for the other. There is thus indicated an increase of regularity with the decrease of absolute error. A comparison of deviations for the several angles shows that the right angle has by far the smallest deviation (about one-fourth of the average), and that the smallest and largest angles are somewhat more regularly reproduced than intermediate ones, and thus again indicate the direct relation between error and regularity.

Finally as to the significance of these results, we may offer the following suggestion. Our curve may be viewed as consisting of two portions, the first beginning with 15° and ending with 75°; the second beginning with 105° and ending with 165°, i. e., we omit the right angle as well as the angle 0°. In that case the curve falls into two (often strikingly) similar portions, beginning with an exaggeration and ending with an underestimation. This would mean that angles with a small excess over 0° or 90° are more exaggerated

or less underestimated than angles with a greater excess over 0° or 90°, and in this special sense is it true that acute angles are underestimated and obtuse angles overestimated; the smallest and largest angles forming an exception to the generalization.

More on Details of Method.—The angles were measured by applying a square of card-board 25 mm. square to the horizontal line, having the apex of the angle coincide with a corner of the square; the distance of the intersection of the oblique line with the side of the square was noted to the nearest ¼ mm. and from this the tangent of the angle could be readily calculated; for the above process gave us the measurement of the opposite side of an angle whose adjacent side was always 25 mm. In this way under favorable circumstances a set of twenty-two angles could be measured and the results tabulated in five minutes. There is inevitably some error in this mode of measuring, and to eliminate such error, as far as possible, we measured our standard angle by the same method, finding as a result the angles 15° 40′, 31° 23′, 44° 25′, 61° 38′, 76° 30′, 90°, 105° 40′, 120° 33′, 136° 10′, 151° 38′, 166° 30′; the deviations plotted in the curves are from these angles and not from the theoretical angles 15°, 30°, 45°, 60°, 75°, 90°, 105°, 120°, 135°, 150°, 165°. In comparing the general average, the average of each individual was weighted by the number of sets of which it was the average.

B.—On the Judgment of the Positions of Lines.

With the assistance of James H. Turner.

Several points in the results just described suggested further research. The fact that one side of each angle was given as well as that the lines are drawn on square pieces of paper with one line parallel to the side of the square, may have important influences upon the results. To eliminate entirely the influences which these conditions may have induced, it seemed necessary to ensure an environment for the subject in which no straight lines whatever should be visible except those judged, for the lines of the floor and walls are manifestly sufficient to give him his vertical and horizontal and thus a basis for estimating angles.

To secure these conditions we arranged the experiment so that the subject could see nothing but one or two white card-board discs, four inches in diameter, upon which was drawn a straight black line three inches long. The two discs, one above the other, were viewed against a large black disc thirty inches in diameter, all placed in a vertical position. Above his head was a parasol-like frame, from which hung black draperies and a similar black cloth was drawn across his lap. When in position he was completely enclosed under this canopy, the light coming in from the back above his head; no portion of the floor or walls was visible to him. He was seated on a chair with his eyes on a level with a point midway between the two discs and about 15-20 inches away from them. The subject's arms reached outside of this canopy and held two handles attached by cords to the axle upon which the upper of the small discs turned. By pulling the right or left cord the subject could thus bring the straight line drawn upon the card-board into any position. This line was three inches long and 1 mm. wide and its centre was the centre of the four-inch disc. At the other end of this apparatus, which was firmly mounted upon a table, sat the observer, who had before him two circles divided to half degrees; to each of the other ends of the axles, upon one end of which was glued a four-inch disc, was attached a "hand" ending in a fine needle point, which was so ad-

justed as to assume precisely the same position as the line upon the disc. This adjustment was constantly regulated by setting the line vertical (by a fine plumb line) and noting the deviation, if any, of the hand from 90°. Finally a third axle midway between the two that bore the four-inch discs and thus in the centre of the thirty inch disc, bore a five-inch black paper disc eccentrically mounted and covering at pleasure either the upper or the lower disc.

A twisted cord attached to the axle and also to a hinged lever, the cord drawn and kept tense by a weight, enabled the operator by a simple movement to conceal either the upper or lower disc. Both were never in view at once. With this apparatus our method of experimentation is very simple. The operator sets the line of the lower disc at any desired angle; he then uncovers this disc, allowing the subject to view it until a clear impression of the position of the line is obtained; he then instantly covers this disc, and the subject, by means of the strings, sets the line of the upper disc to correspond to the remembered position of the lower line. The operator reads the position on his divided circle and the difference in the two readings gives the error. In the meantime another position has been set on the (invisible) lower disc, which is now revealed, and so on. Eighteen positions were used, forming angles of 0°, 10°, 20°, 30°, 40°, 50°, 60°, 70°, 80°, 90°, 100°, 110°, 120°, 130°, 140°, 150°, 160°, 170° respectively with the horizon. The observations were taken in sets of eighteen, each angle occurring once in a set. With this apparatus we studied the error in setting, after a brief interval, one line in the same position as a standard line; this judgment clearly involves angles, for it is based partly at least upon the angles formed with ideal verticals and horizontals. It also involves the conception of parallelism, for the task may be conceived as that of setting one line parallel to another.

Ten individuals were tested, seven of them drawing each position of the line ten times, and three of them each line twenty times. The average settings in degrees and tenths of degrees of each of the ten subjects for each of the eighteen positions of lines are exhibited in the following table. The Roman numbers indicate the different subjects; the upper line, the standard positions of the lines. The average of all is shown in the lowest line:

	10°	20°	30°	40°	50°	60°	70°	80°	90°	100°	110°	120°	130°	140°	150°	160°	170°	180°
I.	9.6	23.5	31.7	37.8	49.0	60.8	69.7	78.7	91.2	104.2	113.7	122.5	131.7	140.9	153.7	162.0	171.5	178.5
II.	12.0	21.1	27.8	41.4	55.5	60.2	68.3	79.2	92.0	104.2	114.4	123.4	129.8	140.8	151.6	162.5	171.0	180.4
III.	10.2	19.9	29.1	41.9	52.0	61.4	74.1	83.8	91.2	103.6	111.9	124.8	131.9	146.6	155.9	167.2	176.2	181.4
IV.	9.6	18.4	28.5	40.5	47.9	55.9	63.9	71.8	90.6	105.2	111.9	121.1	131.2	144.9	153.7	162.5	174.4	180.7
V.	11.9	19.8	30.0	43.3	50.9	57.9	67.6	74.9	90.4	102.2	111.0	121.2	129.7	138.3	150.8	157.2	166.4	179.6
VI.	14.3	24.6	37.8	47.1	54.4	65.3	72.0	82.8	90.9	106.2	114.4	123.6	133.2	141.2	153.1	161.3	172.7	180.6
VII.	10.8	21.9	31.9	39.4	52.4	61.0	72.7	82.9	90.3	101.5	114.0	121.4	131.7	143.2	152.7	162.5	173.0	180.3
VIII.	9.5	24.3	31.6	37.8	49.8	60.6	70.0	82.7	90.8	99.2	110.2	117.1	134.2	141.4	148.7	162.9	174.5	181.7
IX.	9.2	21.1	33.5	44.2	53.6	62.5	73.0	81.0	89.6	100.7	114.8	123.4	132.5	141.8	155.6	161.2	170.3	180.2
X.	10.6	19.6	29.8	39.0	46.0	57.8	66.7	76.1	89.0	105.0	114.7	126.1	133.5	141.9	151.3	158.5	168.9	179.8
Average	10.77	21.42	31.7	41.24	51.4	60.34	69.80	79.39	90.6	103.2	113.1	122.46	131.94	142.1	152.91	161.78	171.89	180.32

If these averages be plotted in a curve similar to that drawn for reproduction of angles, a very irregular curve will result, presenting practically no constant characteristics. The errors vary with each angle and almost with each individual. All the angles are so nearly correctly reproduced that the order of their correctness seems almost accidental, although there are abundant indications that the vertical (90°) and the horizontal (180°) are more accurately reproduced than any others.

It is also true that the angles are rather more apt to be over-estimated than under-estimated, and that the obtuse angles are rather more over-estimated than the acute ones. Of the 180 records entered in the table, 138 are over-estimations and 42 under-estimations. Of the 90 records for angles of 90° and less, 55 are over-estimations and 35 under-estimations; of the 90 records for angles between 90° and 180°, 83 are overestimations and 7 underestimations. To this extent the characteristics of the former results reappear. As a very rough comparison of the errors in drawing angles from memory with one angle and the side of the other given, in doing this with both angles visible, and in judging positions of lines, it may be stated that the average error for all angles (without regard to their being positive or negative) of the three individuals whose records we have for all those methods are 3° 40′ in the first case, 2° 42′ in the second, and 1° 53′ in the third. The entire curve for positions of lines is thus nearer the line of no error than that for reproducing angles. It must be remembered that much of this resulting absence of error is due to the balancing of errors of opposite directions, particularly so with acute angles. The subject had, if anything, a smaller degree of confidence in the correctness of his reproduction of positions of lines than in those of angles, and had decidedly less confidence in the former than in the latter when these were drawn with both angles visible. An indication of the regularity of these reproductions is furnished by the average deviations of the individual reproductions from their mean. This for the three methods (average of all the eleven angles, eighteen positions) is for one individual 2° 50′ for angles from memory, 1° 33′ for angles with the standard angle visible and 3° 15′ for positions of lines; for another subject 3° 05′, 2° 08′ and 3° 49′. This would indicate the greatest variability, least regularity for the estimation of positions of lines, the least variability for reproduction of angles, with the standard angle visible, and an intermediate degree for reproductions of angles from memory. This order corresponds with the subjective feeling.

In general, then, we conclude that in the reproduction of positions of lines without reference to any but imagined coördinates, the absolute error is small; is on the whole an overestimation of the angle;[1] is greater with obtuse than with acute angles; while the individual variation of the results is rather large. No simply formulatable law is followed by the resulting curve of error; the errors varying irregularly with the angle and the individual.

B II.—On the Judgment of Horizontal, Vertical and Oblique Positions of Lines.

With the assistance of W. D. Brown.

It has already appeared that much of our perception of angles and positions of lines takes place by reference to an ideal vertical and horizontal which we constantly carry with us and have had

[1] This overestimation means that the upper end of the line was set too far to the right; this may be due to a greater dependence of the right eye in judging or in the adjustment of the right hand.

forcibly impressed upon us by the countless verticals and horizontals with which civilization has surrounded us. It would indeed be strange if this enormously extensive experience with right angles, verticals and horizontals should not have left its impress upon our psycho-physiological organism. We have had some evidence of it in the accuracy of judging right angles; and the importance of the subject led us to undertake the determination of the accuracy of this ideal vertical and horizontal. We did this with the apparatus above described, using only the upper disc. The subject simply set this disc until the line upon it appeared to him exactly vertical or horizontal. We also had him set it in a diagonal position 45°, with the vertical or horizontal, speaking of these as "left oblique" or "right oblique," according as the upper end of the line pointed to the left or right. Each set consisted of 20 settings, in which the four positions, vertical, horizontal, "left oblique" and "right oblique," occurred in a chance order.

Observations were made upon ten subjects, eight of whom set each line twenty-five times (five sets) and two of whom set each line fifty times (ten sets). In the following table appear for each subject the number of settings of each position, the resulting average for each position and the average variation of the twenty-five (respectively fifty) records from their mean value.

The last line of the table averages these results for the entire ten subjects:

Subject.	No.	Vertical.		Horizontal.		Right Oblique.		Left Oblique.	
	Obs.	Setting.	Variation.	Setting.	Variation.	Setting.	Variation.	Setting.	Variation.
J. J.	50	89° 26′	0° 31.7′	179° 29′	0° 34.4′	42° 02′	2° 29.4′	134° 58′	1° 52.5′
W. D. B.	50	89° 51′	0° 45.3′	179° 19′	0° 30.7′	44° 59′	2° 41.2′	135° 46′	2° 29.0′
F. S.	25	90° 40′	0° 49.2′	180° 16′	0° 35.8′	42° 49′	2° 52.2′	138° 41′	3° 12.1′
F. E. B.	25	90° 42′	0° 40.1′	180° 39′	0° 54.7′	36° 25′	4° 04.4′	141° 25′	3° 58.2′
E. P. S.	25	92° 35′	0° 34.2′	182° 14′	0° 34.1′	44° 08′	2° 36.7′	139° 44′	1° 22.3′
J. H. D.	25	90° 26′	0° 25.5′	179° 52′	0° 51.4′	41° 16′	2° 04.8′	139° 20′	2° 28.2′
J. H. T.	25	88° 53′	0° 36.9	178° 52′	0° 57.5′	38° 40′	2° 52.1′	137° 40′	2° 59.0′
C. M. R.	25	90° 02′	0° 31.2′	180° 13′	0° 36.2′	41° 17′	4° 07.3′	146° 12′	3° 37.2′
G. W. M.	25	90° 09′	0° 30.4′	180° 02′	0° 24.1′	42° 39′	3° 16.6′	136° 42′	4° 09.0′
E. T. V.	25	89° 40′	0° 33.8′	180° 13′	0° 31.4′	33° 41′	2° 08.9′	141° 29′	3° 16.1′
Average.		90° 14′	0° 35.8′	180° 07′	0° 39.0′	40° 50′	2° 55.4′	139° 12′	2° 55.4′

It appears at once that the ideal verticals and horizontals that we carry with us are exceedingly accurate. This is shown not alone by the close approximation to 90° and 180°, but by the very small average variation, less than two-thirds of a degree. The individual variation is also small; 40.8′ for 90° and 36.1′ for 180° amongst the ten subjects. The diagonal positions show a larger and more constant error; the right oblique, if exact, should be at 45°, but, in all cases, it is less than this. The left oblique, if exact, should but be at 135°, but in all cases [in the first line (J. J.) there is an underestimation of 02′] it is overestimated. This means that in both cases the oblique lines were placed too near the position of the horizontal, in the one case (right) by 4° 10′, in the other (left) by 4° 12′. The average variation is also larger than in verticals and horizontals, being nearly 3°. The individual variations for the ten subjects are 2° 42′ and 2° 26′.

No elaborate comment upon these results is necessary. They give evidence of how thoroughly we have been drilled in the perception of rectangular coördinates; and the small variation both of the individual records and of the subjects is especially noteworthy. Our perception of positions midway between the vertical and horizontal is not so accurate nor so constant, the tendency being to approximate them too closely to the horizontal. The error is the same in direction and extent as that for angles of 45° and 135°, when they were reproduced with the standard angles visible (see Fig. 2 above), but is out of relation to the corresponding reproduction of positions of lines.

A Further Study of Involuntary Movements.

With the assistance of Thomas P. Carter and Edward P. Sherry.

In a previous contribution (this Journal, Vol. IV. pp. 398-407) there was described an apparatus—the automatograph—by which involuntary movements in the direction of the attention could be readily recorded; and typical illustrations were given of such movements, obtained under various conditions. In further study of these movements we attempted to determine the effect of the position of the body upon them, to analyze them with their constituent factors, and to experiment upon certain other points closely related to these.

If the arm be extended to the side of the body, movements of the hand forward are more readily made than movements backward, and movements toward the body more readily than movements away from the body. The hand moves most easily along a circumference of which the shoulder is the centre. The desideratum is a position in which movements in all directions would be equally easy; while this is almost impossible to secure, it may be approximated by extending the hand at an angle of about 45° with the line joining the shoulders and with the elbows bent at an angle of about 120°. The hand thus extended is placed upon the centre of the automatograph—firmly fixed to the table—and a constant position is secured by outlining in chalk the position of the subject's feet upon the floor. In this way the differences in question were reduced, but not eliminated; the average of all the comparable records at our disposal shows half again as extensive a movement towards an object of attention to the front as towards one to the rear, and a third again as much movement towards as away from the body. In some cases, too, the tendency to move forward overbalances the tendency to move towards the object of attention; in such cases we should have, however, a smaller movement to the front than when the object of the attention was to the front. In brief, the difference in the records accompanying the direction of

the attention to the front and to the rear, seldom fails to appear, although it may appear as a difference in the amount of movement instead of in the direction of movement. Fig. 5 shows a case of the former kind, obtained under different but comparable conditions; Figs. 7 and 8 illustrate the more usual result. The fact that the movements recorded serve as an index of the direction of the attention may thus be established independently of the influence of the position of the body; a conclusion corroborated by results to be described presently.

Observation of the subject's movements during an experiment strongly indicated that the result was complex, and originated in several portions of the body; it seemed both general and local. The chief factor in the general movement was referred to a swaying of the body with the feet as a pivot; this swaying of the head we recorded by fixing the recording plate horizontally on the subject's head[1] and suspending above it the glass pencil, held in an adjustable arm, which was firmly fixed to an upright on the table. The device for holding the writing point is the same as that used in the automatograph and is shown in full size in Fig 3. A cork (C) is pierced by the snugly fitting glass tube (T) and within the tube the pointed glass rod (R) moves easily up and down, accommodating itself to all irregularities of surface or movement; a small rubber band (B) is useful in raising the pencil off the record and in preventing it from falling through the tube. For the record nothing is better than the small ground glass drawing-frames that children use. These nicely hold and stretch the glazed paper; they may be stacked without injury to the record, and the frame prevents the pencil from leaving the record. This device may be variously used and may be recommended as the simplest method of recording movements of the kind in question. It is to be noted that when the subject holds the record-plate and the pencil is fixed, there is recorded a movement in opposite direction to that really made.

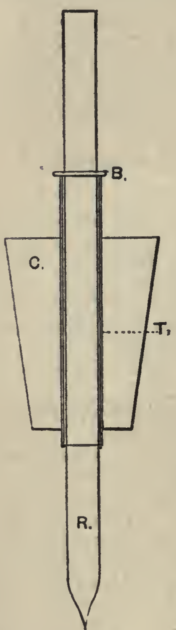


Fig 3. Device for recording movements. The glass rod (R) moves freely up and down in the glass tube (T) held in the cork (C). The rubber band (B) prevents the rod from falling throught the tube.

The movements of the head[2] show the influence of the direction of the attention similarly to those of the hand; indeed the correspondence between the two is considerable and often striking. It appears best when movements of the head and of the hand are recorded at the same time. Fig. 4 may serve to indicate the degree and nature of the correspondence; the head movements are apt to be more extensive and distinctive than those of the hand. This favors the conclusion—to be reinforced by other considerations—that the swaying of the body contributes an important part to the automatograph records.

[1]To fasten this upon the subject's head, a screw eye is fastened to each end of the frame holding the smoked paper; a rubber band is drawn over each arm up to the shoulder; to this band is affixed another rubber band passing through the screw eye and thus securing the frame upon the head. Some soft padding under the frame is desirable.

[2] The records for this study (Figs. 2-12 and 16) have all been taken upon the same individual and thus are as comparable as possible.

We failed to discover any constant tendency to sway in a special direction; movements backward seemed to be as readily made as those forward, and to the right as readily as to the left. When the attention is not directed in any special direction or is directed to a point overhead, an irregular forward and backward as well as lateral swaying results, which is quite different from that accompanying a definite direction of the attention.

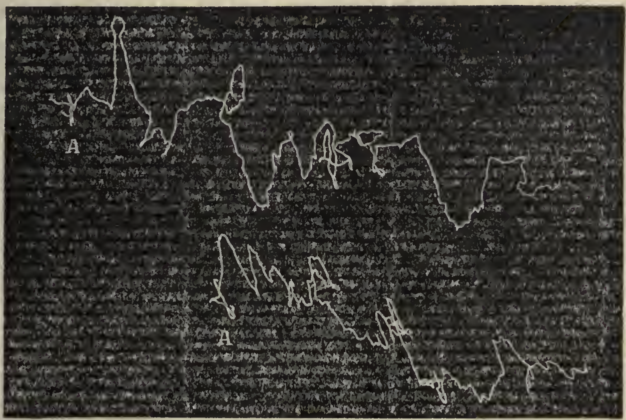


Fig. 4. Counting metronome. ⟶ Upper line, movements of head; lower line, of hand on automatograph; time, 45 seconds. The head movements are reversed, but have been again reversed for readier comparison. Figs. 4 to 13 are all obtained upon the same subject. The arrows indicate the direction in which the object attended to was situated.

The most obvious method of eliminating these swaying movements is to experiment with the subject in a sitting position. A typical record of the hand movement on the automatograph with the attention directed to the front appears in I, Fig. 5. The irregu-

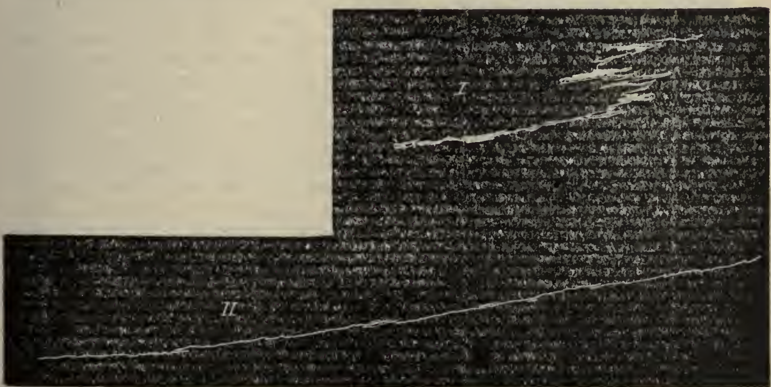


Fig. 5. Counting metronome. Facing ⟶. Automatograph, sitting. I, ⟵; time, 105 seconds. II, ⟶; time, 45 seconds.

lar lateral oscillations have nearly disappeared; the tendency to move along a circumference of which the shoulder is the centre is marked. A more satisfactory method of eliminating the swayings of the body consists of holding the pencil in one hand and the record-plate in the other; in this way pencil and record sway together and thus no record of it is made. Under these conditions we obtain a characteristic type of movement; the several oscillations are small and fine, as appear best when examined with a magnifying glass. Fig. 6 illustrates the type of movement very

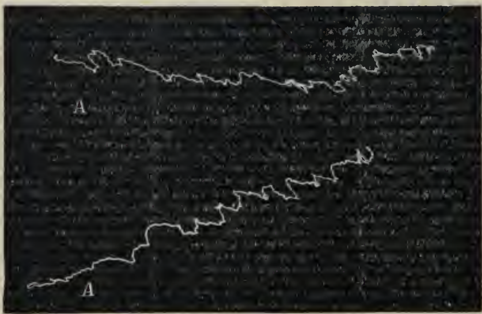


Fig 6. ⟶ Counting metronome. Right hand holds pencil, left hand holds record; time of each, 90 seconds. Facing ⟶. Upper line, standing; lower line, sitting.

well. It further illustrates that by this method there is no difference, or but a slight one, between the records taken while the subject is standing and while sitting; which is precisely what should be the case if the general movements of the body have been eliminated. In this figure traces are observed of a somewhat regular periodic "curve"; these mark the respirations, and in II, Fig. 7, they are sufficiently distinct and regular to be counted, about twenty to the minute. It is natural that the respiration should appear, because the arm holding the record-plate is rested against the body, and thus records the abdominal movements, though these are apt to be obscured by involuntary movements of the hand holding the pencil. The tendency to move towards the object of attention appears throughout; Fig. 7 further illustrates a movement to the rear as well as to the front, and in Fig. 8 we have an unusually clear indication of readiness with which the direction of the attention may be received. The subject attends to and counts the beats of a metronome, which is in turn carried from one corner of the room to the next; the hand accurately follows the attention, yielding an almost perfect square. In all these tests it is important that a position be chosen in which movements in all directions are equally possible.

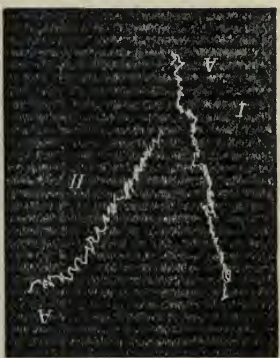


Fig. 7. Thinking of a building. Facing ↑; standing. Right hand holds pencil, left hand holds record; time of each 60 seconds, I, ↑; II, ↓; shows respiration.

As a further test of our analysis of these movements we recorded the movements of the two hands at the same time. This may be done by holding a pencil in each hand over a record-plate placed upon a table, or by holding a record-plate in each hand under a writing point projecting from a table or upright. The difference between these methods is not great; the former method allows of slight finger movements, while the latter does not. The latter is, on the whole, more convenient, because the natural sinking of the hand cannot spoil the record, which might be the case in the other method. The record-plate was placed upon a light board, to which a handle set vertically or horizontally could be attached. Both methods admit of a variety of positions of the arms and hands and dispense with the necessity of maintaining the record-plate level[1]. The results show that the movements of the two hands are very similar indeed; part but not all of this similarity is due to the swaying of the body, which would naturally affect the two sides alike; but there seems also a tendency for the two hands to move together in following the direction of the attention. Fig. 9 illustrates the close similarity of the movements of the two hands. It is important to remember that the records must be made in the same way and the hands be held in the same position. The tendency to move is greater when the hand is held away from than when held close to the body; Fig. 10 illustrates this difference and at the same time shows the correspondence of the form of the movement, notwithstanding the difference in extent.

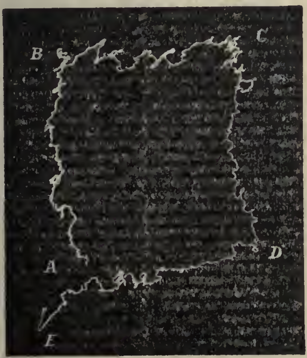


Fig. 8. Counting metronome. Right hand holds pencil, left hand holds record. From A to B, ↑; from B to C, →; from C to D, ↓; ↑ from D to E, ←; standing. Each part, 45 seconds.

We have thus illustrated a variety of methods of recording involuntary movements and of analyzing the chief factors contributing to the result. In a measure this separates the mechanical from the psychological portions of the movements and sheds some light upon the positions and methods used in muscle-reading; the additional facilities derivable from the movements of locomotion should not be overlooked.

To this account may be added a few illustrations interesting from various points of view. Involuntary movements are naturally not limited to the horizontal plane; the rod sliding within the tube simply records these alone. We may fix the record-plate in a vertical position against the wall and take the cork between the fingers of the outstretched hand, holding the tube in a slanting position, and thus record vertical movements. This is, however, a fatiguing position, and the fatigue is manifested in a sinking of the hand and arm. This is usually quite rapid and may readily be

[1]The difference between records made with the automatograph and with the device figured in Fig. 1 held in the hand, is mainly one of extent of movement. The automatograph records more finely the tendency to move towards the object of attention as well as the general movements of the body. The illustrations of this article compared with those of the former show the nature of the difference. A further advantage of the automatograph is that it rests the arm.

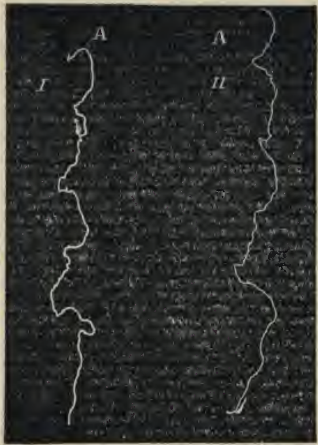


Fig. 9. ↑ Thinking of a building. Standing ↑. ↑ I, left hand; II, right hand; both holding record near the body; time, 35 seconds; records reversed.

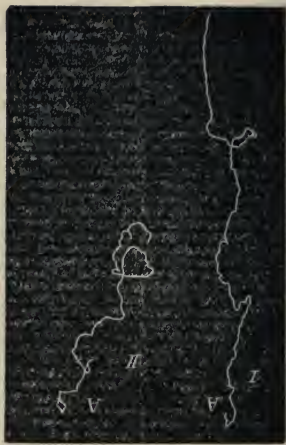


Fig 10. ↑ Thinking of a building. Facing ↓. ↑ I, left hand held extended far out. II, right hand held close to body; each hand holds record; time, 35 seconds; records reversed.

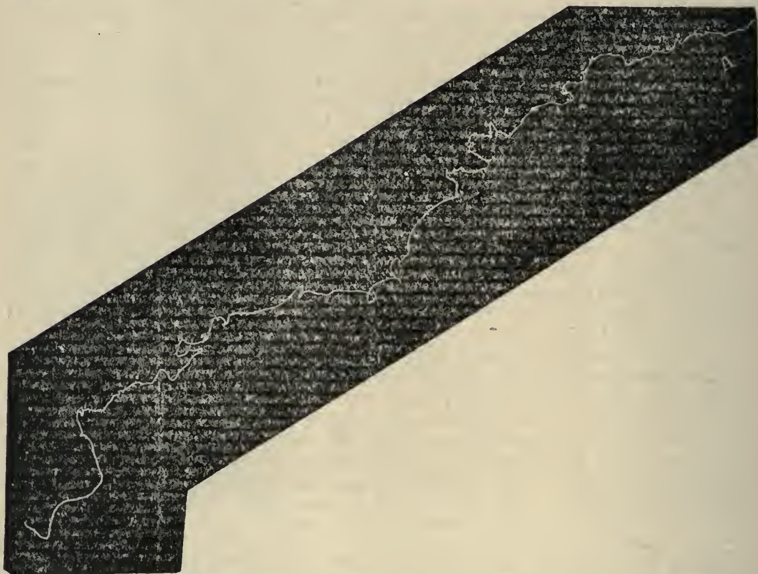


Fig. 11. ⟵ Counting metronome. Record vertical. Facing ⟵. Time, 20 sec. Pencil held in extended right hand.

recorded; an illustration is given in I, Fig. 12. If in this position the attention is directed forward, we obtain a resultant of the two tendencies, as is shown in the diagonal curve of Fig. 11. Fig. 12

further illustrates an interesting point similar to that illustrated in Fig. 5. In curve I the attention is directed downwards, which quickens, though probably not considerably, the natural tendency for the hand to fall; in curve II the attention is directed to a point overhead, and we observe that this tendency almost exactly balances the effect of the natural fatigue and thus yields this peculiarly interesting result.

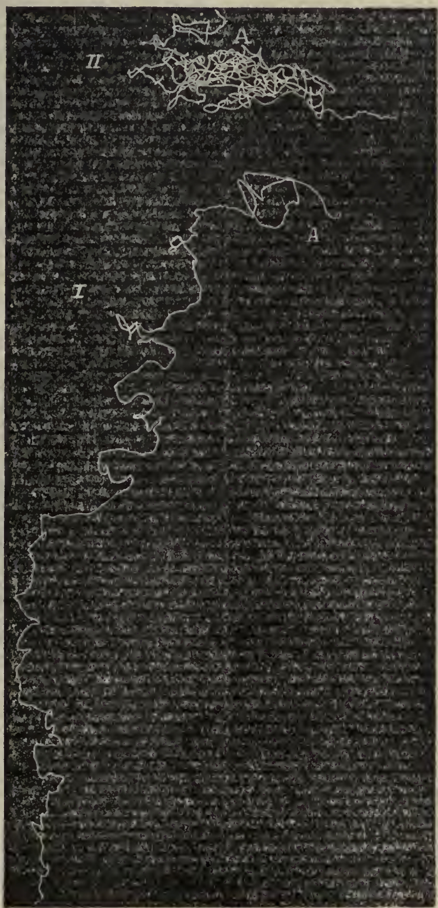


Fig 12. I, ↓ Record-plate vertical. Thinking of one's feet; time, 45 seconds. II, ↑ thinking of a point overhead; time, 45 seconds.

Our attempts to utilize this method for measuring the different degrees of attractiveness of different senses or sense-impressions have not been very successful; and this is mainly due to the great

variability of the result. We have a few illustrations of the difference in question of sufficient interest to reproduce. In Fig. 13

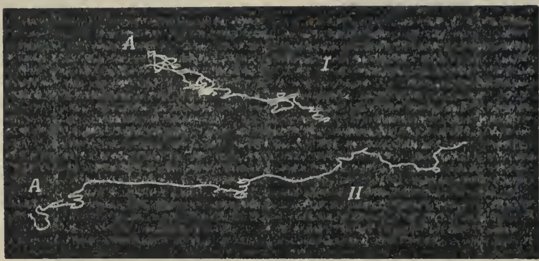


Fig. 13. → I, counting metronome. Automatograph, facing →. Time, 35 seconds. → II, counting pendulum. Automatograph, facing →. Time, 25 sec.

curve I represents the movement of the hand while the subject was counting the strokes of a metronome for 35 seconds; the movement is towards the object of attention, but is slight. Curve II represents the movement where a pendulum is substituted for a metronome, a visual for an auditory impression. In this case the usual impression claims the attention more strongly than the auditory; and this corresponds with the subject's analysis of his mental processes. The subject is a noted American novelist and describes himself as a strong visualizer and in general an eye-minded person.

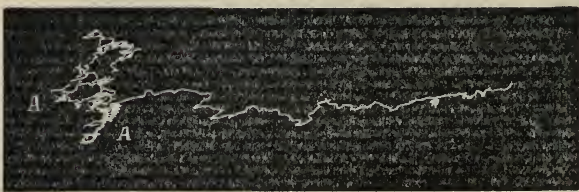


Fig 14. →. Facing →. Hand on automatograph. From A to A′; reading colors; 35 seconds. From A′ on, counting pendulum, 25 seconds.

In Fig. 14 the subject was asked to call the names of patches of colors fixed to the wall opposite him; he did this uncertainly for 35 seconds, his hand moving from A to A′; at this point the counting of a pendulum was substituted for the reading of the colors with a markedly different result, the hand moving directly and rapidly towards the pendulum. Upon examination it proved that the subject's color vision was quite defective, so that the colors did not hold his attention, while the pendulum did. The difference is too marked to be accidental, and is certainly most interesting.

An interesting problem upon which further research is contemplated is the correlation of types of involuntary movements with temperament, age, sex, individual, health and disease, and the like. A few observations were made upon children; they show at once the limited control children possess over their muscles and a similar difficulty in fixing the attention as required. They thus yield an exceedingly irregular result, showing very extensive and coarse

movements, usually towards the object of attention, but with great oscillations. Fig. 15 may serve as a type; in 35 seconds the hand

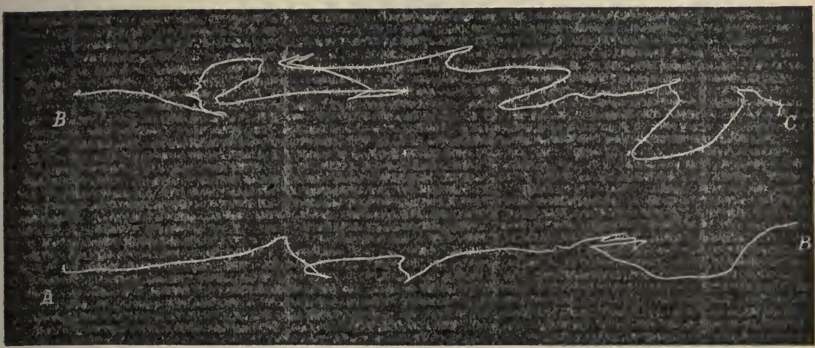


Fig. 15. ⟶ Hand on automatograph. Facing ⟶, counting pendulum. Time, 35 seconds. The record from B′ to C is continuous with that of A to B. The subject, a child of eleven years.

has moved by large skips *seven* inches toward the pendulum, the oscillations of which the child was counting. The difference between this record and those obtained upon adults is striking enough to warrant further study.

Much attention has recently been paid to the subject of automatic writing; in this the subject unconsciously gives indication not of the direction but of the nature and content of his thoughts. We made a few attempts to obtain such upon the automatograph, but entirely without success; we asked the subject to think of a certain letter or simple geometric figure and examine the record for any trace of the outline of this letter or figure. While this method yielded no trace of such a result, it gave us a valuable "control" over the movements in which the attention was directed in a definite direction. In Fig. 16 the subject was thinking of the

Fig. 16. Thinking of letter O. Pencil in hand; record on table. I, standing; II, sitting.

letter O; this was not thought of as in any special location and the record likewise fails to show a movement in any special direction. Two records are shown, one while standing and one while sitting, and these will show the difference in the general and local movements in the two cases; the subject is the same as in Figs. 4-12. This adds the corroboration of a negative proof to the general interpretation of the results.

Further Studies of Classification-Times.

With the assistance of Geo. W. Moorehouse, E. C. Bolton and E. T. Johnston.

This term was applied in the last series of these studies (this Journal, Vol. IV. p. 411) to the time occupied in referring a word or object to its class. The special problem studied related to the time required for answering by the word "noun," "verb" or "adj." (for adjective) when one of ten known nouns, verbs or adjectives was called; i. e., to refer a familiar word to its proper part of speech. This was done when the words were either *nouns or verbs, nouns or adjectives, verbs or adjectives, and nouns, verbs or adjectives.* The times were measured by aid of a "mouth-key," in which the release of a bit of wood held between the teeth started or stopped a chronoscope. The natural opening of the teeth in speaking thus served to mark the limits of the time measured. A simple reaction was obtained by answering always with the sound "*eh*" (violent explosive) when the voice of the observer was heard; the action of repeating the word heard was regarded as involving essentially all the steps involved in the classification-time, except that needed for mentally making the classification. In order to bring our former results in direct comparison with those about to be described, we repeated the same set of reactions upon F. E. B. and E. T. J. as were last year made upon the other two observers. The following table gives (in $\sigma = .001$ second) the simple reaction-time; the repetition-time; the classification-time of nouns and verbs, of nouns and adjectives, of verbs and adjectives; the average of these three; and the classification-time of nouns, verbs and adjectives. Each entry represents the average of about 12 sets of 20 reactions each. The lowest line gives the general average of all the results.

Subject.	Simple Reaction.	Repetition.	Noun-verb.	Noun-adjective.	Verb-adjective.	Average of N.-V., N.-A. and V.-A.	Noun-verb-adjective.	Difference between Simple and Average of N.-V., N.-A. and V.-A.	Difference between Repetition and Average of N.-V., N.-A. and V.-A.	Difference between Average of N.-V., N.-A. and V.-A., and N.-V.-A.
J. J.	190	367	599	595	593	596	667	406	229	71
G.W.M.	195	280	628	597	679	635	678	440	355	43
F. E. B.	201	364	579	623	604	602	663	401	238	61
E. T. J.	179	366	612	619	611	614	684	435	248	70
Aver.	191	344	604	608	622	612	673	421	268	61

From this table we may gather that it takes 191σ to signal by a vocal reaction that a sound has been heard; 344σ to repeat a spoken word; the difference between these 153σ for the processes of distinguishing the sound and calling into play the proper vocal utterance; 612σ to refer a spoken word to one of two grammatical

classes, there being no difference amongst the three pairs of parts of speech used; 673σ or 61σ additional to do this when the spoken word may be one of three classes of ten each; and 268σ for the mental process of deciding and recalling what the appropriate part of speech is.[1]

Our next step was to substitute for the grammatical classes the three classes of *animals*, parts of the human *body*, and articles of *dress;* as before, ten distinctive and familiar monosyllabic words were chosen as follows: *dog, fox, hen, pig, cow, bee, snake, goose, goat, horse; eye, ear, nose, mouth, head, arm, hand, foot, tooth, thumb; hat, cap, coat, vest, glove, shoe, boot, tie, cuff, shirt.* We answered "*an*," when the name of an animal was spoken; "*bod*," for parts of the body; and "*dress*," for articles of dress. As before, we classified these words when they were selected from *animals* and parts of the *body*, from *animals* and *dress*, from *body* and *dress*, and from *animal*, *body* and *dress;* and in all respects repeated for these classes what he had done for the grammatical ones. Our results are embodied in the following table, in which each entry is the average of 12 sets of 20 reactions each:

ANIMALS, BODY, DRESS.—*Verbal Series.*

Subject.	Animal-body.	Animal-dress.	Body-dress.	Average of A.-B., A.-D., B.-D.	Animal-body-dress.	Difference between Simple and Average A.-B., A.-D., B.-D.	Difference between Repetition and Average of A.-B., A.-D., B.-D.	Difference between Average of A.-B., A.-D., B.-D., and A.-B.-D.
J. J.	747	768	693	736	809	546	369	73
G. W. M.	567	575	551	564	637	369	284	73
F. E. B.	724	709	683	705	783	504	341	78
E. T. J.	658	694	673	675	737	496	309	62
Aver.	674	686	650	670	741	479	326	71

Several conclusions drawn from the former table reappear in this one. The three kinds of classifications with two classes take about equal times; the additional time needed to make the same classification when the word may be one of three classes is about the same. Further, the process of classifying words as *animals*, *body* or *dress* is longer than classifying them as parts of speech for three subjects, for J. J. by 140σ, for F. E. B. by 103σ, for E. T. J. by 61σ, but 71σ shorter for G. W. M. G. W. M. experienced

[1] It is also noted that our additional research entirely confirms the conclusions drawn in the last study, viz., that the results for J. J. are more typical than the others. The peculiar difficulty of G. W. M. to distinguish verbs from adjectives still affects the results. But the main irregularities in our former results, due to irregularities of practice, do not reappear, owing to the fact that care was taken that the various kinds of reaction be equally distributed throughout the work.

unusual difficulty in naming the parts of speech of words, and found the classification with *animal*, *body*, *dress* somewhat easier than the others; it is, perhaps, fair to regard the average of the other observers 102σ (or an increase of 17%) as representing the increased difficulty involved. It is easier upon hearing the word dog, to recall and say that *dog* is a *noun* than that *dog* is an *animal;* which would in turn indicate that we have been better schooled in recognizing the parts of speech of words than in recognizing the more or less natural classes into which the objects denoted fall.

Our three groups of ten words each were chosen with reference to easy pictorial illustration, for our design included the classification of the pictures of these objects as well as of the spoken words. For this purpose pen and ink drawings of the thirty objects were prepared and fastened upon small slips of glass; the drawings were of a uniform size, the extreme outlines being contained within a circle 35 mm. in diameter. A frame was made in which ten of these pictures could be mounted in a carriage and moved along horizontally in back of a shutter such as the photographers use. This shutter consisted of two wings in back of an opening of adjustable size; a pressure on an air bulb withdrew the wings from the opening in the usual way, and in so doing established an electric circuit by which the chronoscope was started. By the aid of a series of spring stops and a weight to move the carriage, we could conveniently and quickly bring any one of the ten pictures behind the opening in the shutter. The reaction was made as before by speaking the appropriate class name with the mouth-key. After each ten reactions the pictures were changed and two series of ten reactions each constituted a set. The following table, arranged similarly to the former tables, gives the results of our "pictorial" series. Each entry under "simple" and "naming" represents the average of 12 sets of 20 observations each; each entry under the other columns of 8 such sets.

ANIMAL, BODY, DRESS.—*Pictorial Series.*

Subject.	Simple.	Naming.	Animal-body.	Animal-dress.	Body-dress.	Average of Animal-body, Animal-dress, Body-dress.	Animal-body-dress.	Difference between Simple and Average of A.-B., A.-D., and B.-D.	Difference between Average of A.-B., A.-D., and B.-D. and A.-B.-D.
J. J.	202	484	524	526	570	540	603	338	63
G. W. M.	214	522	547	494	532	526	605	312	79
F. E. B.	235	563	563	561	577	567	639	332	72
E. T. J.	185	558	545	507	524	526	589	341	63
Aver.	209	532	545	522	551	539	608	330	69

Again it appears that the classification of a word into any one of the three pairs of classes requires about equal times, and that the

additional time to do this for one of three classes remains the same, about 70σ.

The chief result of a comparison of this with the former table is the fact that it takes *less time to classify a picture than a word;* less time to recall and say that "dog is an animal" when the picture of a dog is shown than when the word is spoken. While the classification in the verbal series (for two classes) requires 670σ, in the pictorial series it requires only 539σ, or 131σ less; for classification into three classes, 741σ and 608σ, or 133σ less. It is fairer to take account of the differences in the simple reaction-time of the verbal and pictorial series, 191σ and 209σ; and thus the difference in the mental processes of classification is greater by 18σ than the differences just given.

In the verbal series we found reasons for regarding the time of repeating a spoken word as involving all the processes of classifying the word except the act of recalling the classification (see Vol. IV. pp. 412–413); the pure mental classification time for the grammatical series (two classes) would thus be 268σ, for the verbal animal-body-dress series 326σ. In the pictorial series we were unable to devise any means of making this elimination, and so cannot say how much of the difference between the simple reaction and the entire classification-time, 330σ, is taken up by the process of recognizing and indicating the recognition of the picture, how much by the recalling of its class name. There are strong reasons for believing that very much the greater portion of the 330σ is taken up in the mental classification process.

It is further of interest to compare the process of classifying with that of naming. Is it a more complicated process, upon seeing the picture of a dog, to say "dog" or to say "animal"? Do we first recognize the lines as representing a dog, and then decide that a dog is an animal, or do we at once recognize the drawing as that of an animal? We are able to give but an imperfect answer to this question. For J. J. it is easier to name than to classify, and the time is shorter by 56σ; for G. W. M. and F. E. B. there is practically no difference; for E. T. J. naming requires 32σ longer. The inferences from these results are that the two processes are about of equal complexity, that it is unlikely that the specific recognition of the class to which the object belongs includes the recognition of the individual object, and that the processes may be different in different persons.

We have already noted that the three pairs of distinctions of classes are of equal difficulty; it is further of interest to ascertain whether it is easier to pronounce a word a noun, verb or adjective; a name, a picture, that of an animal, of a part of the body, or of an article of dress. The following table gives the data for this decision, and by noting the numbers in bold type we see that on the average of all cases in which a word was pronounced a noun, the time required was 616σ, for verbs 627σ, for adjectives 651σ; for animals 695σ, for body 690σ, for dress 698σ; for pictures of animals 555σ, for body 568σ, for dress 564σ. In each class the three types of classification are thus of practically equal difficulty. The increase of time needed to pronounce a word an adjective above that needed to pronounce it a noun is of note, however, amounting to 35σ in the average and appearing in each of the four individuals. The individual records agree well with their average. The table will doubtless be clear without further comment:

Subject.	Noun.				Verb.				Adjective.			
	N.-A.	N.-V.	N.-V.-A.	Aver.	N.-V.	V.-A.	N.-V.-A.	Aver.	N.-A.	V.-A.	N.-V.-A.	Aver.
J. J.	604	594	631	610	594	568	651	604	596	618	698	637
G. W. M.	620	568	657	615	636	669	675	660	627	689	701	672
F. E. B.	594	626	610	610	564	604	652	607	620	604	728	651
E. T. J.	609	612	665	629	615	613	683	637	626	607	704	646
Average.	607	600	641	616	602	613	605	627	617	629	708	651

Subject.	Animal.				Body.				Dress. (Words.)			
	A.-B.	A.-D.	A.-B.-D.	Aver.	A.-B.	B.-D.	A.-B.-D.	Aver.	A.-D.	B.-D.	A.-B.-D.	Aver.
J. J.	744	764	806	771	750	687	806	748	772	701	814	762
G. W. M.	566	547	628	580	568	549	632	583	603	555	651	603
F. E. B.	723	739	743	735	725	683	804	737	679	683	803	722
E. T. J.	657	680	742	693	659	667	746	691	708	679	723	703
Average.	674	682	730	695	675	647	747	690	690	655	748	698

Subject.	Animal.				Body.				Dress. (Picture.)			
	A.-B.	A.-D.	A.-B.-D.	Aver.	A.-B.	B.-D.	A.-B.-D.	Aver.	A.-D.	B.-D.	A.-B.-D.	Aver.
J. J.	521	517	581	540	527	569	619	572	535	571	610	572
G. W. M.	557	501	592	550	537	514	620	564	485	550	592	542
F. E. B.	561	562	622	582	567	576	635	593	560	578	661	600
E. T. J.	554	495	592	547	536	528	589	551	519	522	587	543
Average.	548	519	597	555	542	547	616	568	525	555	612	564

It is also true that it takes practically the same time to name a picture of an animal, a part of the body, or an article of dress. This appears in the following table:

Naming Pictures of Objects.

Subject.	Animal.	Body.	Dress.	Average.
J. J.	488	469	494	484
G. W. M.	530	513	524	522
F. E. B.	573	569	546	563
E. T. J.	557	558	558	558
Average.	537	531	527	532

The final average (532σ) represents the average of 960 reactions, animals being named 340 times, parts of the body 266 times, and articles of dress 354 times. We also append a statement of the number of errors, i. e., in which a word or picture was referred to a class not its own. Our data for nouns, verbs and adjectives relate only to F. E. B. and E. T. J.; for these the numbers of errors in percentage of observations taken are as follows:

SUBJECT.	N.-A.	N.-V.	V.-A.	N.-V.-A.	Average.
F. E. B.	1.67	6.17	6.17	2.92	4.27
E. T. J.	2.92	1.67	1.67	1.25	1.88
Average.	2.30	3.92	3.92	2.09	3.08

For the verbal series the errors in percentage are:

SUBJECT.	A.-B.	A.-D.	B.-D.	A.-B.-D	Average.
J. J.	7.92	5.42	3.33	5.83	5.77
G. W. M.	8.92	12.92	12.92	14.54	12.29
F. E. B.	5.00	2.92	5.42	2.50	3.96
E. T. J.	4.58	6.17	5.00	1.45	4.38
Average.	6.61	6.86	6.67	6.08	6.60

Average of F. E. B. and E. T. J., 4.17.

For the pictorial series the errors in percentage are:

SUBJECT.	A.-B.	A.-D.	B.-D.	A.-B.-D.	Average.
J. J.	1.25	3.75	0.63	0.67	1.56
G. W. M.	10.00	0.63	3.75	8.12	5.62
F. E. B.	0.63	1.25	1.87	1.25	1.25
E. T. J.	0.63	0.63	1.87	2.50	1.41
Average.	3.13	1.57	2.03	3.12	2.46

Average of F. E. B. and E. T. J., 1.33.

It appears that the percentage of error is smallest for the pictorial series, largest for the verbal series, and intermediate for the grammatical series. The individual difference that should be noted is the large number of errors of G. W. M., which is undoubtedly related to the shortness and mode of his reactions. The order of the four subjects regarding their liability to error is the same in the

verbal and pictorial series. It is also of interest to inquire whether the average time of these erroneous reactions is markedly different from the time of the correct ones. In the following table the average time of the erroneous reactions is given in percentage of the corresponding average correct reactions; and the results show, on the whole, no appreciable difference between the two. In the erroneous reactions there is probably a greater variation than in the correct ones:

	J. J.	G. W. M.	F. E. B.	E. T. J.	Average.
Grammatical Series.	. . . . .	. . . . .	94.8	119.5	107.1
Verbal Series	108.6	97.6	99.9	103.1	102.3
Pictorial Series.	97.2	105.3	107.0	97.2	101.7

On the Perception of Simultaneous Sense-Impressions.

With the assistance of George W. Moorehouse, Fellow in Psychology.

The error in indicating with which one of a series of visual impressions an auditory or other impression seems simultaneous, was first noted by Wundt. He studied it by having an index rotate in front of a graduated disc at a constant rate, or again by having it oscillate with a pendular movement, and noting to what stroke of the disc the hand seemed to be pointing when a bell sounded. The actual moment of the sound was determined by the observer by moving the pendulum slowly across the disc and the error in time was then calculated by mathematical formula. With this apparatus Wundt established that it takes many separate judgments before one is ready to make one's decision; that the error is very variable; that the error for almost all the rates of movement used is negative, i. e., the time at which the bell is said to have rung precedes the time of its actual ringing; that this error decreases as the speed increases, until it becomes positive; that the error increases in the accelerating portion of a pendular movement, and decreases in the portion of diminishing velocity; that the error with the constant motion disappears when one division[1] of the disc corresponded to $\frac{1}{36}$ of a second ($=28\sigma$) and the interval between successive sounds of the bell is one second; and finally the very important fact that the determination was considerably under the control of the will of the observer, and was influenced by the direction and nature of the attention.

Wundt also experimented with more than two simultaneous impressions, but his results on this point need not now be considered.

Tschisch (Wundt's Studies II. 603-634) has contributed an elaborate research, working with the same apparatus, but his main results are concerned with the determination of the error with several simultaneous impressions. Reference to his results will be made later on. Both Tschisch and Wundt connect with their results an elaborate theoretical interpretation.

[1] This division refers to the smallest portion of the divided circle taken into account in the subject's judgment; Wundt's apparatus as figured has a mark for every two degrees, but it is to be inferred that he judged only to the nearest ten degrees. The importance of this point is the subject of discussion below.

For several reasons a reinvestigation of the fundamental factors of these interesting phenomena seemed desirable; the accepted interpretation of the error as the time needed for the reception and elaboration of the perception (*Complication einer Vorstellung*[1]) seemed questionable; the dependence of the error upon the apparatus as well as upon the mode of judgment seemed not to have been sufficiently regarded. The phenomenon, when reduced to its simplest terms, may be thus described: There is a series of sense-impressions following one another in a recognizable order and the members of which are distinguished from one another both in time and by some other characteristic; a disparate and momentary sense-impression is interposed at some moment unknown to the subject, and he must determine with which one of the series of impressions the disparate impression seemed to coincide. In order to take note of small errors, it is necessary that the successive members of the series of sense-impressions be rapidly distinguished; and sight and hearing alone, therefore, are available for this purpose. It is true that we can distinguish both these and other sense-impressions by the artificial device of counting, but this process is too slow and absorbs too great a share of the attention to be here available. Sight is decidedly the preferable sense by reason of its superior power of taking in a large range of impressions at once; and in many ways the most convenient visual impressions are the divisions of a divided circle. The place of a given mark in the circle is readily determined when each fifth or tenth mark is differentiated from the others; the circle used by Wundt has a short stroke for each two degrees and a larger stroke for each ten degrees, and this division we have used in our experiments. A point travels along this visual scale, and for the interposed impression the stroke of a bell or an electric shock on the finger is most convenient. Our problem then is simply this: Where upon the divided circle was the moving point when the bell sounded or the shock was felt? The most important factor in this decision is obviously the accuracy with which the subject is required to decide; i. e., whether he is to determine the point when the bell sounded to the nearest ten, five, two or one degree; this is the one point that must be determined before the observations can proceed, and indeed must be considered in the preparation of the divided circle. And yet it is a surprising fact that this is the one point upon which former observers have been most reticent; one can only infer it, and that not too certainly from the apparatus used. Wundt, in his observations with an index revolving at a constant rate, judged to the nearest ten degrees; Tschisch apparently judged more accurately, and if by a division (Theilstrich) he means a division of Wundt's apparatus as figured, he judged to the nearest two degrees. The accuracy of the judgment with a given rate of movement is dependent upon the size of the divisions; after a certain velocity has been reached, we can no longer distinguish the several positions of the moving point. The larger the disc, the higher the speed as measured by the time of one revolution at which an interval of a given number, say two degrees, may be distinguished. Wundt's disc for constant movement was but slightly over six inches in diameter, and thus it is clear why he could judge only to the nearest ten degrees; in the apparatus of Wundt used by Tschisch, the disc is about 8⅜ inches in diameter.

[1] Tschisch has elaborated and Wundt has endorsed this interpretation as applied to more than two simultaneous impressions; and a table is given indicating the time of the several higher kinds of reception and fusion of perceptions (Tschisch p. 633; Wundt, 3rd Ed. II. p. 341).

To obtain a greater range of velocity of movement we used a much larger disc, 22 inches in diameter, divided by short strokes into two degrees, and by a longer one for every ten degrees. We judged not only to the nearest stroke, but also whether the point stood on or between two strokes when the bell sounded, i. e., we *judged to the nearest degree.* The distance on our disc between two strokes (two degrees) was 9.75 mm., or about $\frac{3}{8}$ of an inch; in Wundt's disc this distance was only 3.84 mm., about $\frac{3}{20}$ of an inch. Furthermore, to secure ease of reading, the markings were plain and bold, in black ink upon white card-board, and the index was blackened and tapered to a readily visible point.

In almost all previous determinations, the index moved with a pendular movement; while it is interesting to observe the effect of the change of velocity upon the error of judgment, it is certainly important to have determined the error for a constant rate as a standard of comparison; and to regard the rate at the base of a pendular oscillation as equivalent for this purpose to a constant rate is not free from objection. In all our observations, the index traveled over the disc at a constant rate.

This disc was glued to a board, 22 inches square, and the whole mounted in a vertical position; through a hole in the centre of the disc, the axle bearing the index projected and this index could be set in any position and then fastened by a thumb-screw. The mechanism by which the index was rotated was the clock-work of a clinostat. This apparatus was admirably adapted to our purpose and admitted of a great range of velocity. On the axle, behind the disc, was fastened a small wire, the end of which just dipped into a mercury drop, and thus in each revolution established an electric circuit. By this connection, a bell could be struck or an electric shock given to the finger, and by the setting of the index the point at which this occurred was charged. Moreover, a switch in each circuit enabled the observer to introduce the sound or the shock at any desired moment; this is important, as no judgment should be made until the clock-work has obtained its full and constant rate. The subject sat at a convenient distance before the disc, the latter concealing from his view all the mechanism by which the index was rotated as well as the bell, and called out the positions at which the sound or shock seemed to come.

A further point in the method of observation is of importance. The judgment in which the subject has any confidence is formed only after several observations, the point at which the impression was interposed shifting with each observation. There are two natural methods of recording the error; the one is to take the *average of all* the observations with a given setting of the interposed impression; the other is to ask the subject to decide upon *one judgment as the final* one, and to measure the error by this, recording, however, the several observations as well. After a trial of both, we adopted the latter plan as the better. The point at which the interposed impression really occurred was readily determined by slowly moving the index (by turning with the finger one of the fine wheels of the clock-work) until the bell sounded or the shock was felt. In order to have the sound or shock as brief as possible, the mercury cup was made in the form of a narrow slit, through which the point of the wire could be made to pass at any desired angle, and to prevent the sound of the bell from continuing after it was struck, the bell was loaded with drops of wax. In some cases we found it more convenient to use a spring wire instead of the mercury drop. The rate of the index was determined by timing to the nearest second three, five or ten revolutions, according to the

rate, and the result was expressed uniformly *in the σ=.001 of a second required for the point of the index to travel over one degree of the circumference.*

The results we have thus far accumulated are in every sense provisional; the number of observations is not adequate and they are offered at the present time simply because of the interest in the methods by which they have been obtained and their explicit disagreements with previous results.

We calculate from the tables of Tschisch, that with the index moving at such a rate that 1° was passed over in 3.07 σ, the error for sound was a negative one of 64.8 σ, for touch 64.8 σ, for electric shock 72.2 σ; with a faster rate of 1° in 2.41 σ, these errors were 44.1 σ, 44.1 σ and 39.9 σ; and with a still faster rate of 1° in 1.7 σ, these errors were 20.3 σ, 20.3 σ and 20.3 σ. In all cases these are the errors at the base of the pendular movement, when the acceleration is zero; and by a negative error is meant an error in judging the interposed impression as occurring in advance of its actual occurrence. The sound was that of a bell, the touch a tap of a hammer upon the frontal surface of the last joint of the forefinger and the third kind of stimulus was an electric shock, presumably upon the finger. Tschisch does not describe his manner of recording the judgments, whether he averaged all the observations or accepted a final judgment with each test; in what way he combined errors of opposite direction and the like. The chief characteristics of his result are the large size of the errors; the decrease of the error with an increase of speed and that, too, within small range; the constancy of this error with different kinds of interposed stimuli, and the negative character of the error throughout.

In our observations, the individual variation of the results is so very great that it seems somewhat strained to attach any importance to the general average. These variations are so great that in all the observations with any one rate of speed, observations with positive as well as negative errors occur. Furthermore, within the range of velocities studied by Tschisch, we can distinguish no constant tendencies at all, and within the very much larger range of velocities at our disposal nothing that could be dignified by the name of law appears. Taking all our observations together we find for the sound :

Rate,	1° in 2 to 4 σ.	1° in 4 to 6 σ.	1° in 6 to 9 σ.	1° in 9 σ or more.
Error,	—10.6 σ	+7.0 σ	+5.9 σ	+1.8 σ

These numbers are based upon 120 observations in all.

With the electric shock as the stimulus the results are :

Rate,	1°=2 to 4 σ.	1°=4 to 6 σ.	1°=6 to 8 σ.	1°=8 σ or more.
Error,	—15.8 σ	—3.5 σ	—6.4 σ	—45.1 σ

These results are based upon 130 observations in all.

These results, though entirely provisional and without much significance, owing to the great individual variations, yet are opposed to all the four main results of Tschisch's experiments. What this opposition means, it would be premature to say. But three points, further, need be noticed: (1) the relative constancy of the results when calculated without regard to their positive or negative characters; (2) the difference of individual observers in these observations; (3) the tendency of the several individual judgments in a single observation. With regard to (1) it is only necessary to indicate this fact: this error is not far from 30 σ for the sound, and from 40 σ for the electric shock, independently of rate. (2) We have tested a sufficient number of individuals to make great differences in the size and direction of the error, but not enough to describe them in quantitative terms. With regard to (3) we can only say that with

some individuals the index nearly always moves to a later point of the disc with successive sounds of the bell or shocks on the finger.

We did not confine our studies to this method of observation, but devised several others, to the description of which we may now turn.

We arranged a method by which a series of auditory impressions could be substituted for the visual ones. This arrangement requires the services of three persons; in one room there is the subject with his finger on the "shock key," listening to the reading of the observer; the observer, in reading, speaks into the mouth of the transmitter of a telephone, and at the other end of the telephone in another room sits the recorder; there is, further, an arrangement by which, either automatically or at the desire of the recorder, the shock may be given and the telephone circuit broken. The subject notes at what word he was listening when the shock came and the recorder records the last word heard before his telephone was cut out of the circuit. A simpler mode of observation consisted in connecting the shock circuit with the telephone circuit and noting between what words the slight sounds accompanying the making and breaking of the shock circuit came. We then measure the rate of speaking and calculate the error in σ. In reading from a book, the subject does not know what is coming; but in observing the movements of an index before a disc, the sequence of impressions is foreknown. We can secure the latter conditions for hearing by counting, or by speaking the alphabet. A further variation consists in having the subject himself count aloud or read aloud from a printed page; but this is not so serviceable as the other form of experiment. The result of all our experiments with the auditory series may thus be expressed: The error in indicating the place of a shock in an auditory series is less than one of the smallest units of time (the time needed to speak one word or one syllable) that we could take into account in the observations. The quickest sense-impressions that one can follow by ear is counting from one to ten repeatedly, but this can hardly be done more rapidly than seven per second; our result then simply shows that the error is rarely as large as one-seventh of a second $= 143\ \sigma$. It is further to be noted that the recorder's method of noting the error is not as objective as is to be desired. In noting the place of the two clicks in the auditory series, there is some though less opportunity for the same error of time-location as in placing the position of a shock in the same series. The experimentation is difficult, the results indicating that the ear cannot differentiate and locate the sequence of impressions with sufficient rapidity to permit of the detection of the error under consideration.

As a further contribution to the influence of the apparatus and mode of judgment upon these errors of location in time, we altered one of the most important conditions of all former experiments: instead of having the disc stand still and the index moved, the reverse was done. This was accomplished as follows: Upon a revolving drum was fastened a sheet of paper with various lines of letters, words and numbers written upon it with a type-writer; the drum was in a horizontal position, but to bring the letters in a vertical position and to have them pass across the field of vision from right to left (and thus be read from left to right) as well as to have but one line, or rather as much of one line as one could see, in sight at any one time, two mirrors were appropriately placed at the end of a shallow box, through which the subject read. A fine thread placed in front of one of these mirrors served as an index, the subject judging what letter or number was opposite the thread when

the bell sounded or his finger received the shock. As before, the drum moves at a constant rate, and a final judgment is recorded after several individual observations. We judged always to the nearest letter or number (there were 135 in a line), and in some instances we attempted judgments between or upon letters or spaces, that is, to the nearest half letter. It will be seen that a letter corresponds to $2\frac{2}{3}^\circ$ or a half-letter to $1\frac{1}{3}^\circ$. We used 12 different lines; lines I. and II. were continuous words from a story for children; line III., a series of detached monosyllabic words; line IV., miscellaneous numbers between 20 and 100; line V., the numbers in regular order from 20 on; line VI., the numbers advancing by 7 from 12 to 100, 11 to 100, 13 to 100; line VII., letters of the alphabet in chance order; line VIII., a line of verse; line IX., of prose; line X., the same line of prose, but in reversed order; line XI., a line of German; line XII., a scale of short uniform marks with every fifth mark heavier and numbered to correspond exactly with the divided circle with the rotating index.

Let us consider line XII. first, as that allows of most direct comparison with former results. With this method of judging a much slower rate is necessary; the circle is much smaller, about 4 inches in diameter; it is more difficult to read the lines while in motion, and a smaller portion of the circle is visible at any one time.

With the bell our results are :

Rate,	1° in 20 to 24 σ.	1° in 24 to 28 σ.	1° in 38 to 42 σ.	75 observations.
Error,	—27.6 σ	—20.7 σ	—28.8 σ	

With the electric shock, the results are :

Rate,	1° in 20 to 24 σ.	1° in 24 to 28 σ.	1° in 32 to 50 σ.	60 observations.
Error,	—50.7 σ	—23.2 σ	—54.4 σ	

The great variability of the results is again a striking factor, though they are almost uniformly negative. There is no definite connection suggested between rate and error, and the error is different with the bell and with the shock.

The other eleven lines were arranged to furnish material for the study of the effect of the different kinds of visual series upon the error. Most of the observations were, therefore, made with one rate of speed, about 1° in 20 σ. If we divide the lines into those containing continuous words, I., II., VIII., IX., XI., those containing detached words or letters, III., VII., X., and those containing numbers, we find as the general average error of the first set $+2.2\sigma$, of the second $+7.0\ \sigma$, of the third, $0.0\ \sigma$. There is no difference of note between the results of bell and shock. The small error and great variability of the results are again the marked characteristics; the averages have thus little significance. But one further result is worthy of notice. If, instead of recording simply the final judgment, we record all the individual answers and enter their average as a result, then the error seems to be larger; we can make such a comparison for the "shock-stimuli," though the two sets of results were not taken at the same rate (the "average" judgments being at about half as rapid a rate as the "final" ones). In the first case the average error is $4.1\ \sigma$ and in the second case $35.7\ \sigma$. Attention is again directed to the provisional character of the results throughout, and to the fact that our main objects are the analysis of the factors involved in these observations, the indication of the dependence of the results upon method, apparatus and mode of judgment, and the recording of the absence of agreement of our provisional results with those obtained by other observers.

The Psycho-physic Series Applied to Lifted Weights.

With the assistance of Walter D. Brown.

The method of the psycho-physic series presents to the subject a range of sensation intensities—usually successively one at a time—and requires him to assign each sense-impression to one of a given number of divisions or magnitudes. By this method we may compare the assigned magnitudes of the stars with their photometric intensities and determine whether the subjectively equal different magnitudes (arithmetical series) correspond to a similar objective series or—as the psycho-physic law demands—to a series of equal ratios (geometrical series). In former contributions (Vol. I. pp. 112-127, Vol. III. pp. 44-79, Vol. IV. pp. 213-219) the close correspondence of estimated star-magnitudes with the law was shown, and the method applied with varying results to sensations of visual and tactual expression, to the time-sense and to the motor-sense. A comparison of all the results suggested the generalization that the law probably holds of sensations that are appreciated *en masse*, without conceiving them as divided into units, on a general unanalyzed impression.

Our present study applies this method to the sensations obtained in lifting weights in the palm of the hand, and this includes the sense of muscular contraction as well as the pressure-sense of the palm. Sixty weights were prepared, the lightest weighing 12 grms. and the heaviest 795 grms.; the intermediate weights corresponded to the average of an arithmetical and geometrical series inserted between these limits; in this way the selection of weights, while from the subject's point of view essentially a matter of chance, favored one result no more than the other. A set consisted of 60 observations, each weight being assigned once to a magnitude. The subject, without distinctly seeing the weight, lifted it up and down in his right hand and assigned it according to his sensations, to one of six magnitudes or classes. The lightest weights were grouped as Class I., the heaviest VI. The order of the weights was determined by chance. The weights were made by packing cylindrical boxes $3\frac{1}{8}''$ high and $1\frac{5}{16}''$ in diameter with leaden discs cut to fit the inside of the box and supplemented by felt discs, cotton and shot; the weight was equally distributed throughout the box and were all alike in appearance, being marked by a letter on the bottom. Two sets (120 judgments) were taken upon seven subjects and four sets (240 judgments) upon three subjects. The average weight in grammes and the results of all the weights assigned to each of the six magnitudes or compartments I., II., III., IV., V. and VI. by each of the new subjects is given in the following table, the last line giving the average of all:

Subject	No.	I	No.	II	No.	III	No.	IV	No.	V	No.	VI	No.
J. H. T.	120	39.4	23	103.2	23	175.4	14	249.2	14	370.1	18	619.4	28
F. S.	120	43.9	23	92.3	23	178.9	16	257.2	14	400.8	21	640.4	23
J. H. D.	120	25.8	13	71.2	17	146.0	28	286.0	22	438.5	20	664.3	20
F. E. B.	120	27.3	14	74.6	19	131.6	18	240.4	24	391.8	21	640.2	24
E. P. S.	120	23.6	19	91.1	20	181.5	28	323.4	17	461.7	19	696.2	17
C. M. R.	120	34.5	20	97.6	23	170.7	18	285.2	23	433.9	15	660.0	21
E. T. J.	120	40.5	26	127.8	29	239.3	20	369.2	16	526.2	14	676.1	15
G. W. M.	240	27.0	30	58.0	41	148.2	39	232.6	36	370.2	45	624.3	49
W. D. B.	240	29.5	30	75.8	34	145.1	46	255.8	44	408.9	40	643.8	46
J. J.	240	28.2	30	76.2	39	151.4	47	266.1	45	422.6	28	620.1	51
Average	1560	32.0	228	86.8	268	166.8	274	276.5	255	422.5	241	648.5	294

In the next table are given for each subject, in the upper line, the successive *differences*; in the lower line, the successive *ratios* between the average weights of neighboring magnitudes; in the last three columns are found the averages of these differences and of these ratios, the average deviation of the several differences and of the ratios from their mean (expressed in percentage), and the ratios of these percentages of deviation to one another. In the lowest lines of the table, similar results are given for the general average of all.

Subject	I-II	II-III	III-IV	IV-V	V-VI	Average	Average Deviation	Ratio
J. H. T.	63.8	72.2	73.8	120.9	249.3	116.0	47.7%	1: 2.51
	2.62	1.70	1.42	1.49	1.68	1.79	19.0%	
F. S.	48.4	86.6	78.3	143.6	239.6	119.3	48.4%	1: 3.56
	2.10	1.94	1.44	1.56	1.60	1.73	13.6%	
J. H. D.	45.4	74.8	140.0	152.5	225.8	127.7	42.4%	1: 2.37
	2.76	2.05	1.96	1.53	1.52	1.96	17.9%	
F. E. B.	47.3	57.0	108.8	151.4	248.4	122.5	50.4%	1: 2.77
	2.73	1.77	1.82	1.63	1.63	1.97	18.2%	
E. P. S.	67.5	90.4	141.9	138.3	234.5	134.5	35.7%	1: 1.89
	3.90	1.99	1.77	1.43	1.51	2.12	18.9%	
C. M. R.	63.1	73.1	114.5	148.7	226.1	125.1	39.8%	1: 1.93
	2.82	1.75	1.67	1.52	1.52	1.85	20.6%	
E. T. J.	87.3	111.5	129.9	157.0	149.9	127.1	17.4%	1: 0.62
	3.15	1.87	1.54	1.43	1.28	1.85	28.3%	
G. W. M.	31.0	90.2	84.4	137.6	254.1	119.5	53.8%	1: 2.97
	2.15	2.55	1.60	1.59	1.69	1.91	18.1%	
W. D. B.	46.3	69.3	110.7	153.1	234.9	122.9	46.3%	1: 2.86
	2.57	1.91	1.66	1.60	1.57	1.86	16.2%	
J. J.	48.0	75.2	114.7	156.5	197.5	118.3	39.7%	1: 2.17
	2.66	1.99	1.76	1.59	1.47	1.91	18.3%	
Average	54.8	80.0	109.7	146.0	226.0	123.3	40.6%	1: 2.04
	2.71	1.92	1.65	1.53	1.53	1.87	19.9%	

The general result is indicated in the last entry in the column of ratios; *the approximation to a geometrical series is, on the average, twice as close as to an arithmetical series; to this extent the psychophysic law is followed.*

Passing to the individual results, it is seen that all the individuals, with one exception (E. T. J.[1]), favor the geometric series, and of these nine, six approximate it more closely than the general result. If we omit the one divergent record, the general deviation from an arithmetical series becomes 43.4%; from a geometrical series, 17.8%, and their ratio as 1: 2.44. We have, then, a coarsely approximate geometric series, but one which presumably is fairly constant in different individuals.

In the application of the method of the psychophysic series to the time-sense, it was found that the first set of those subjects upon whom more than one set was taken, conformed much more closely to a geometrical series than did the following ones. The same is true of only one of the three subjects who contributed two sets to the present study. For W. D. B. the ratio of approximation to a geometrical series to that of an arithmetical series, as in his first set, 1: 5.27; in his second set, 1: 1.60; in the two combined, 1: 2.86; for G. W. M. these ratios are 1: 2.83; 1: 2.42; and 1: 2.97; for J. J., 1: 1.63; 1: 1.90; and 1: 2.17. This would indicate that practice has less tendency to change the method of judging lifted weights than of time-intervals; in the latter case the approximation to a geometric series is much closer than in the former.

It will be observed that the deviation from a geometric series proceeds, not in a hap-hazard way, but exhibits a fairly definite and constant tendency. The ratio between the average weights of neighboring magnitudes is not a constant, but decreases by smaller and smaller steps, and thus approaches a constant. The unusually high ratio between the classes or magnitudes, I.-II. is a common characteristic of such results (see this Journal, Vol. I. p. 123, Vol. IV. p. 216) and is in large measure accounted for by the fact that the number and average weight of observations falling in Class I. are affected by there being no class smaller than I., to which doubtful judgments might be assigned. The decline of the ratios was in the case of the star-magnitudes expressed by an empirical formula, making the ratio a constant multiplied by a constant times the excess of the magnitude, above a given magnitude. The ratio is expressed, not by a straight line, as it would be if it were a constant, but by a line inclined to the horizontal at a slight angle. A similar inclination, though not a constant one, is suggested by the present results.

The suggestions offered in former applications of this method are entirely corroborated by the present study; the tendency to have equal ratios of objective stimuli correspond to equal sensation-differences is strong and natural in such types of sensation as are estimated grossly and from an impressionist point of view, without reducing them to units or conceiving them as thus reduced. We are quite likely to gauge weights by an unanalyzed feeling of effort, which we do not tend to reduce to pounds and ounces, and this is the natural basis of the psychophysic law. We reserve for a future contribution the general discussion of all the results thus far obtained in the application of the method of the psychophysic series to various types of sensation.

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[1] In this case there are reasons for believing that the subject took a very artificial view of the problem before him, and more or less consciously favored the arithmetical series.

NOTE UPON OTHER RESEARCHES.

To complete the account of the studies of the year, mention may be made of a few studies, as yet incomplete, or to be published elsewhere. In collaboration with Mr. Geo. W. Moorehouse, a new æsthesiometer has been devised, which differs in essential points from those now in use. It permits of testing the sensibility of the skin with a variable pressure upon the points of the skin tested; the motion by which the points are applied is constant, regular and simple. Furthermore, a series of attachments is to be constructed by which the same apparatus may be used for exploiting the various types of tactile sensibility, for the pressure sense, and for the temperature sense; the apparatus will thus test all the chief sensibilities of the skin. The construction is not elaborate and the cost will be moderate.

Two researches of a statistical nature have been undertaken and are nearing completion. One is a study of the dreams of the deaf, with a view of determining the effect of the age of becoming deaf upon the future retention of "dream hearing," and of recording many other peculiarities of the dreams of this class. Mr. E. T. Johnson has had charge of the tabulation of this interesting but troublesome material. The other is a study of association and community of thoughts; the main point being to determine in what degree different persons are apt to think of the same association when starting from a common point, and then following their own line of association. A word is given to a class of students, and at the same time each member of the class writes the first five words suggested by the original word. The proportion of similarity of association in all the first words written, in all the second, etc., as well as in the sum total of all the words is the chief point to be studied; and the main result is the regular decrease of community of association as the words are removed from the original word. The first words suggested to different persons by a given word are more apt to be the same than the second, the second more so than the third, and so on.

STATISTICS OF "UNCONSCIOUS CEREBRATION."

By Charles M. Child, Wesleyan University.

The present article is an attempt to give in a statistical form the results obtained from a set of questions on "Unconscious Cerebration." These questions were first issued by Mr. Francis Speir, Jr., of South Orange, N. J., and a part of the results of his investigation were published in the *Popular Science Monthly*, Vol. 32, p. 657, under the title, "*The Antechamber of Consciousness*." In order to the continuity of the inquiry it seemed best to re-issue the same set of questions, and these Mr. Speir very kindly furnished, together with the answers which had been returned to him. These answers were mostly from students of various colleges, and from persons in professional life. To these were added about a hundred more, all of college students, making the whole number of answers two hundred. These latter were collected by Professor A. C. Armstrong, Jr., of Wesleyan University, under whose direction the present investigation has been carried out. From these answers the statistics have been compiled, first, in general, with no regard to sex or age or other conditions; then the sexes were separated and the percentages for each were obtained; and third, the percentages were computed for the different ages.

In the two hundred papers there are one hundred and fifty-one from men and forty-nine from women. As regards age, the greater portion of the persons answering are between twenty and thirty years, and more of these are under twenty-five years than above. As the papers naturally fell into several divisions according to age, it seemed advisable to separate them as follows: first, those under twenty-five years; second, those between twenty-five and thirty years; and third, those over thirty years of age. The number of persons in each division is as follows: ninety are under twenty-five, thirty-two are between twenty-five and thirty, and forty-one are over thirty, besides which there are thirty-seven who do not give their ages.

Before giving the statistics a few words of explanation may be necessary. Each question is given separately, and following it are the percentages, together with any examples or remarks. A part of the examples quoted here are from the papers furnished by Mr. Speir and a few of them are given in his article; the others are from the papers collected by Professor Armstrong. In explanation of the figures it may be said here that in the tables the horizontal series headed "whole number answering," those answering "no" and "indefinite" have been computed only in the general division, and, when it is not otherwise stated, are percentages of the whole number of answers returned, i. e., two hundred. The figures in the different divisions headed "men," "women," etc., are, unless it is otherwise stated, percentages of the number of persons in each division. All the percentages are given as whole numbers, fractions of one per cent. being discarded. This sometimes causes a slight apparent discrepancy, as, for example, that noted below under the

first question. With this explanation and the notes given with each table, the figures will doubtless be clear. Only a part of the questions are given in tabular form, as it was unnecessary to give all the figures in every case. In the questions as given below the original order has been somewhat changed, and some portions, as well as some entire questions, which elicited answers of no essential value to the subject, have been omitted. The questions omitted are those numbered I., V. and XI. in Mr. Speir's original list. In other respects our list is identical with the original.

QUESTION I. 1. When you are unable to recall the name of something wanted and you say, "Never mind, it will occur to me," are you conscious of any effort of searching after it?

2. When you are, do you feel some trouble or weight in your effort?

3*a*. Does the idea ever seem to have come back spontaneously without being suggested by any perceived association of ideas?

b. Does such recovery of the lost idea ever come during sleep?

c. Does such recovery come after sleep?

d. Please give examples from your own experience, illustrating fully.

	1	2	3*a*	*b*	*c*
Whole number answering.	93	81	92	86	83
Those answering no.	21	12	11	68	31
Indefinite.	0	0	0	4	6
	Yes.	Yes.	Yes.	Yes.	Yes.
General.	72	68	81	17	57
Men.	72	68	77	18	60
Women.	73	67	84	15	54
Under 25 years.	72	72	79	10	51
Between 25 and 30 years.	78	66	84	33	56
Above 30 years.	66	61	71	21	65

In the first two columns there is an apparent discrepancy due to disregarding fractions of one per cent. In the first column the general percentage is 72, that of the men 72, while that of the women is not, as the general percentage would seem to indicate, 72, but 73. The same variation is seen in the second column. The percentages in the vertical columns under *b* and *c*, with the exception of the first three in each column, are percentages of the number of those in each division who answer 3*a* affirmatively.

In the answers to 1 there is little variation except in the last two divisions. Those between twenty-five and thirty show a distinct rise, and those above thirty a fall in their percentage. Under 2

those under twenty-five are above the general percentage, those between twenty-five and thirty somewhat below, and those above thirty still further below. In 3*a* the women show a higher percentage than the men, and here there is again the distinct rise between twenty-five and thirty, while those above thirty are considerably below those under twenty-five. Under *b* a somewhat larger percentage of men than of women answer affirmatively; the percentage of those between twenty-five and thirty is about double the general percentage, while that of those under twenty-five is less than the general percentage. In *c* also the percentage of affirmative answers is larger among the men than among the women. Here there is an increase in the percentage of affirmative answers with increase of age.

A few examples, which are among those given in the papers under the general subject of the spontaneous recovery of ideas, may be interesting and are given below.

1. "This morning I endeavored to recall the name of the characters I had read of in one of Scott's novels the night before. I could remember but one, and then only with much effort. During the morning I was unable to recall any other character by name, although constantly endeavoring to do so. After teaching a Sunday-school class, I walked home in the afternoon with my mother, and, without any effort, gave not only the names of the principal characters but many of the unimportant. I had not thought of the work for a number of hours."

2. "I was trying to think of the name of a book, and gave it up. About half an hour after, I was talking of something else when, all of a sudden, I blurted out the name without any conscious volition on my part, or without thinking anything about the book at all."

3. "I have tried to think of the name of a person without success in the evening, and the next morning have had it come to me without any connecting ideas at all, but it just seemed to 'pop' into my mind."

4. "I was telling my sister of a young lady, but I could not remember her name, though I thought I knew it. At last I had to give it up, and after a while forgot all about it, though I could not at first force myself to think entirely of other things. For a time I was dimly conscious of trying to remember. The next morning the name suddenly flashed across my mind, apparently without being suggested by anything else."

Many other examples are given, and a number state that the phenomenon is of very frequent occurrence. Several of the answers give empirical schemes for recalling the lost ideas, such as running through the letters of the alphabet, or working up from connected ideas to the one required.

QUESTION II. 1. Can you wake precisely at a given hour determined upon before going to sleep, without waking up many times before the appointed time?

2. If you can, (*a*) is this habitual, or do you often fail?

b. Are you conscious before waking of any feeling (describe it)?

c. Do you come directly from oblivion into consciousness?

	1	2a		b	c	
	Yes.	Seldom fail.	Often fail.	Yes.	Directly.	Gradually.
General	59	69	25	30	64	16
Men	62	69	28	33	56	16
Women	51	68	12	20	80	16
Under 25	68	66	33	33	62	15
Between 25-30	47	73	20	33	60	13
Above 30	61	68	12	16	64	20

Those who answer 1 are ninety-one per cent. of the whole number; those answering 1 in the negative, thirty-one per cent.; those answering indefinitely, one per cent. The percentages in the first vertical column are computed on the whole number in each division. The percentages in the other five columns are computed on the number of those in each division who answer 1 in the affirmative.

As regards the general percentages, the table shows that fifty-nine per cent. of those sending in papers possess the power of waking at a given time without being disturbed before. About two-thirds of these seldom or never fail in their attempt. Only about a third of them are conscious of any feeling as they wake, and about two-thirds wake directly. In the other division the important points appear to be as follows: A smaller percentage of women than of men possess the power of waking at a given time. Those of both sexes between twenty-five and thirty years are also far below the general percentage in the possession of this power. Those under twenty-five are above and those over thirty are about equal to the general percentage. There is a distinct decrease with increasing age in the percentage of those who often fail in their attempt to wake at a given time. A smaller percentage of women than of men wake with any special feeling, and those above thirty only about half as often as those below. A very large percentage of women wake directly, while men are rather below the general percentage. A larger proportion of persons above thirty than of those below wake gradually. The feeling of which some are conscious on waking is variously described, but is in nearly all cases a troubled feeling, as some describe it, "a feeling that I must wake," "that something must be done," "that it is time to get up," etc. In answering *c* some of those who say they wake directly have a very distinct feeling at the time of waking, so that *b* and *c* are not mutually exclusive.

A few of the examples given are quoted.

1. "Yes, at an early or unusual hour, by repeating the time to myself once or twice before going to sleep. I seldom wake before the hour determined upon and never fail then."

2. "I was intrusted by the attending physician with the administering of medicine to my wife, who was very dangerously ill. It was of the greatest importance that a certain medicine should be given every two hours as exactly as possible. I am an extraordinarily sound sleeper, but for six weeks I woke up every two hours

methodically, and never missed giving the medicine. I always came directly from oblivion into consciousness. I was as exact and methodical during the first few nights as at the last."

3. "I have never overslept when my mind was charged before retiring."

4. "I can always wake at any hour I desire, usually a few minutes before."

5. "Always can wake just five minutes before the hour at which I set the alarm."

6. "I recall one instance more remarkable than any other in my own case. I had been broken of my rest every night for a week or ten days, and one evening retired at about nine o'clock, giving directions to be called at twelve o'clock. I fell asleep at once, and slept till twelve without waking. At that time something seemed to tell me it was twelve o'clock. I seemed to come from perfect oblivion to perfect consciousness. I rose and dressed just as the clock struck twelve. I was under the impression that some one had called me, and was surprised to learn that no one had spoken to me.

QUESTION III. 1. When perplexed at your progress in any work (mathematical, professional, literary, chess, puzzles, etc.), have you ever left it unfinished and turned your attention to other things, and after some time, on voluntarily returning to it, have found yourself able at once to satisfactorily master it?

2. If you have, please give instances.

The answers to the first part are as follows: Ninety per cent. answer the question, seventy-seven per cent. affirmatively and twelve per cent. negatively. Of the men seventy-seven per cent. answer affirmatively, while the percentage in the case of the women is eighty. Those under twenty-five show a percentage of eighty-four answering affirmatively, those between twenty-five and thirty, eighty-one, and those above thirty only seventy-three, a distinct decrease with increase of age. About sixty-four per cent. of those answering are able to give examples of such an experience, while many others say they are sure they have observed something similar, but cannot recall instances.

A large number of the examples given relate to mathematics, a considerable number to the translation of foreign languages, and some to other work, such as essays, puzzles, etc. Some of the examples given will serve as illustrations:

1. "Often while playing chess or working an example I have not succeeded well. On returning after having left it for a while, what was difficult before seemed now very easy."

2. "In working mathematical examples in the evening I sometimes 'get stuck.' I leave it over night and take it up in the morning, and I often get the answer immediately. So in translation I find passages that I cannot get out. I study on them for a while and then leave them for several hours, or better sometimes days, and I can get them clearly."

3. "In writing music I often get to a stumbling-block, and try vainly to search for a chord or bar of music, but cannot find the thing I want. When it gets me very excited I leave it and go for a walk, and on coming back to work, I will most likely be able to write it out at once, seemingly without any work on my part; it is all ready for me to put down. I have frequently had the experience."

4. "I have come across a sentence that was particularly difficult in some Latin book I was reading, and have been unable to translate it. I have then turned my attention to abstruse problems in mathematics, and worked for some time. On returning to the

Latin I have often found it quite simple, and have sometimes translated it at sight."

QUESTION IV. 1. During sleep have you ever pursued a logical, connected train of thought, upon some topic or problem, in which you have reached some conclusion, and the steps and conclusion of which you have remembered on awakening?

2. During a half sleep?

3. If you have, how does the result appear when measured by your normal standard of day-time mental activity, with regard to accuracy, etc.?

4. Please give examples illustrating your meaning in full.

The general answers are as follows: Ninety-three per cent. answer the first section of the question, fifty-nine per cent. have had or recall no such experience, while thirty-one per cent answer affirmatively. The second section is answered by eighty-two per cent., fifty-four per cent. in the negative, and twenty-four per cent. in the affirmative. Seventeen per cent. state that the results appear about as good or better than those reached in waking life, while eighteen per cent. reach conclusions which are far less accurate or absurd.

In the other divisions there is little variation, so it is unnecessary to give all the figures. There are, however, one or two points worthy of note. Only twelve per cent. of the women remember having any logical or connected train of thought in a half sleep, but the general percentage is twice as large. The low percentage of the women here may be connected with the fact that a very large percentage of women wake directly, as was shown in the fourth section of the second question. On the other hand, twenty-four percent. of the women reach results which are at least fairly accurate, this being somewhat above the general percentage, which is seventeen. The percentages of the different ages do not vary far nor with any regularity from the general percentages, and are not given.

Examples under this question are given by forty per cent. of those who have had an experience in sleep or in a half sleep; the following are quoted as showing the degree of accuracy sometimes attained:

1. "I have played a game of chess in my sleep. The game seemed in my sleep to be entirely completed. In the morning I remembered all but one or two plays, and when I played the game over in the morning it seemed consistent. I do not think that I had ever played that game (i. e., a game with those identical moves) before, and I could not play it now. I had been playing a great deal at the time, though, and of course had been thinking of chess when I went to bed."

2. "I have been puzzled by a problem in algebra which I found it impossible to solve, and let it rest over night, and while asleep have thought out each step and remembered it, and in the morning on trying the problem again, solved it without difficulty."

3. "Being greatly troubled over a problem in algebra just before going to sleep, and leaving the problem half finished, I dreamed the rest of the solution and obtained the correct result. On awaking, I remembered it, and it was correct."

4. "In my senior year at college I had an essay to write that troubled me unusually. After trying to decide upon the subject until quite late, I fell asleep and dreamed not only of the subject and analysis, but of all the details. The next morning I wrote out what I had dreamed, and found it far more satisfactory than anything I had ever done in the same line before."

"Two years before I had exactly the same experience about an equation in algebra which I worked out correctly in sleep."

5. "Have worked out many algebraic or geometrical problems during sleep. Have, when some years ago in Worcester Academy, scanned some fifty or seventy-five lines of Virgil not yet translated, except ten or fifteen lines; felt tired, went to bed, in sleep accurately translated all of it, and remembered it on waking."

6 "One evening had been working late on a hard geometry problem, and had failed to solve it. The next morning on awaking I remembered having dreamed of doing it and of obtaining the correct solution. I immediately went over the solution as I had in my dream, and found my reasoning all correct. If I had not thought of my dream immediately on waking up should probably have forgotten my solution, for it was even then hard to recall it."

7. "I had earnestly been trying to make a trial balance and had at last left off working, the summary of the Dr. and Cr. sides of the account showing a difference of £2 10s. 0d., the Dr. side being so much smaller. The error I had not found on Saturday night when I left the counting-house. On this same Saturday night I retired, feeling nervous and angry with myself. Some time in the night I dreamed thus: I was seated at my desk in the counting-house and in a good light; everything was orderly and natural, the ledger lying open before me. I was looking over the balance of the accounts and comparing them with the sums in the trial balance sheet. Soon I came to a small account having a debit balance of £2 10s. 0d. I looked at it, called myself sundry uncomplimentary names, spoke to myself in a deprecating manner of my own eyes, and at last put the £2 10s. 0d. to its proper side of the trial balance sheet, shut up and went home. Here the dream abruptly ended. I arose at the usual Sunday time, dressed carefully, breakfasted, went to call on some young lady friends, and to go to church especially with one of them. Suddenly the dream flashed on my memory. I went for the keys, opened the office, also the safe, got the ledger, turned to the folio my dream indicated. There was the account whose balance was the sum wanted, which I had omitted to put in the balance-sheet where it was now put, and my year's posting proved correct."

QUESTION V. 1. Have you ever been conscious of having discovered something new, e. g., an invention, a literary or poetical creation, or a mathematical solution, etc.?

2. If yes, then has this flashed into consciousness in the form of a clear conception?

3. How many instances can you give?

Seventy-two per cent. answer the first section of the question, forty per cent. negatively, and thirty-two per cent. affirmatively. Of those answering affirmatively, seventy-one per cent. have the idea flash into consciousness in a clear and distinct form. The percentages of both sexes are like the general percentages. Twenty-eight per cent. of those under twenty-five years of age think they have made such a discovery, thirty-two per cent. of those between twenty-five and thirty, and thirty-seven per cent. of those above thirty. Sixty-eight per cent. of those under twenty-five who have made such a discovery state that it came as a clear conception, seventy-five per cent. of those between twenty-five and thirty so state, and sixty per cent. of those above thirty.

These answers show, as might be expected, an increase in the number of such discoveries with increase of age. The percentages of those who answer the second section show an increase between twenty-five and thirty, while above thirty the percentage falls

below either of the others. Perhaps this may be due to the greater ability of the adult's fully developed mind to seize upon a hint as a basis, and work out from it the new idea.

A few of the examples given are quoted.

1. "I can instance as frequent the smallest kind of literary creation, forms of verbal expression, what one may call an apt phrase coming to my mind suddenly, uncalled for, as if uttered by some one else, of no use to me at the time or perhaps ever."

2. "Many instances of mathematical or psychological problems have suddenly flashed across my mind when on a totally different subject; sometimes very distinct and sometimes indistinct, which I afterwards developed into distinctness."

3. "Have often awaked with part of an essay all ready, with a letter wholly prepared; once or twice with a few stanzas composed on subjects that I had endeavored to treat in rhyme, once or twice also on subjects that I had not attempted or thought to write upon in verse."

4. "In one case I wrote a long piece of a rather satirical character, in easy rhythm, as fast as I could set down the words, and it needed little or no revision. Usually I am dissatisfied with my first copies."

QUESTION VI. 1. On seeing a sight (e. g., on visiting a strange place) or on hearing a sound (e. g., yourself or another making a remark), have you ever felt that you had under previous identical circumstances experienced the same before?

2. If you have, then give instances.

3. Describe any general feeling that accompanies this flash of half intelligence.

	1 Yes	3 Describe some feeling.
General	59	67
Men	56	65
Women	71	71
Under 25	69	68
Between 25 and 30	41	92
Above 30	63	50

Eighty-eight per cent answer the first part of the question, twenty-seven per cent answering negatively, and fifty-nine per cent affirmatively. The percentages in the second vertical column are computed on the number of those in each division who answer 1 affirmatively.

As regards the first part of the question, women show a larger percentage in the affirmative than men. Those under twenty-five show a larger percentage than any other age, and one which is above the general percentage, while those between twenty-five and thirty show a much smaller percentage than any other age. In the third section the women again show a higher percentage than the men; those of both sexes between twenty-five and thirty give a very high percentage, and those above thirty are much below the general percentage. The answers to this section vary greatly as regards the nature of the accompanying feeling. Many call it a feeling of annoyance, perplexity or surprise. Some say they have almost a feeling of awe, and one or two call it uncanny.

Sixty-three per cent. of those who recall such an experience are able to give examples. The greater part of these relate to sights or sounds, i. e., remarks, etc., heard or scenes visited, pictures seen and the like. Two persons, however, state that they have had this experience in connection with the sense of smell, but do not give definite examples. A few of the instances related will serve to show their nature:

1. "I have purchased a Chinese umbrella-stand which I know I never possessed before, nor can I recall ever having seen one like it. Yet it is impossible for me to see it without feeling that I have previously owned and used it."

2. "When driving over a new road in a part of the country where I had never been before, and of which I had never seen pictures, it seemed as though I had been over it before under perfectly identical circumstances."

3. "On meeting strange people, a word or look will convince me that I have seen the same thing done by the same person in similar circumstances."

4. "Sometimes I find places which seem to be places I have seen before, and I often find them to be places I have dreamed about."

QUESTION VII. 1. Do you dream?

2. If you do, then (*a*) give any characteristic peculiar to dreaming on the right side, left side and back.

b. Are you conscious of a moral sense during sleep?

	1	2*a*	*b*
Whole number answering	96	79	78
Those answering no	2	26	24
Indefinite	0	22	6
	Yes	More or worse on back	Yes
General	94	31	48
Men	93	28	46
Women	94	39	54
Under 25	98	25	47
Between 25 and 30	87	50	57
Above 30	85	29	43

All the percentages in the second and third vertical columns of figures are computed on the number of those answering the first section of the question affirmatively. The first four in each column are computed on the number in the general division answering 1 affirmatively, and the other five on the number answering in each division respectively, the first section affirmatively.

The sexes show no appreciable variation from the general percentage in the first section. The second section shows that the dreams of women are more affected by position than those of men, and the third section shows that a larger percentage of women than of men are conscious of a moral sense when dreaming. In the answers from those of different ages, there is a continuous decrease with increasing age in the number of those who dream. Those under twenty-five are least affected by position. Those over thirty show a somewhat higher percentage, and those between twenty-five and thirty give a percentage nearly double that of the other two divisions. Again, in the answers to the third section of the questions, there are more between twenty-five and thirty who are conscious of a moral sense than in any other division, while the percentage of those over thirty years of age is the lowest of all.

Besides these answers, two persons state that their dreams are unpleasant when they sleep on the left side as well as when on the back, and one person says he dreams less when lying on the back than in any other position. Two others answer that any unusual or cramped position seems to produce disagreeable dreams.

QUESTION VIII. 1. Do you talk in your sleep?

2. If you do, answer from your family report:

a. Are you able (if accosted when so talking) to answer intelligently questions put to you? If yes, then (*b*) do you answer any questions, or only questions on the subject that you are talking about?

	1	2*a*	*b*	*b*
	Yes	Yes	Any	Only on subject
General	40	37	27	43
Men	41	32	25	55
Women	37	56	30	20
Under 25	58	42		
Between 25 and 30	19	17		
Above 30	19	37		

Eighty-nine per cent. of the whole number answer the first section, forty-eight per cent. negatively, and forty per cent. affirmatively. The percentages in the first column are computed on the number of persons in each division; those in the column headed 2*a* on the number in each division who answer 1 affirmatively, and the two columns under *b* on the number in each division who answer 2*a* affirmatively.

The figures show that a rather larger proportion of men than of women talk in their sleep, while the percentage of women who answer questions when asleep is much larger than that of the men. About twice as many men answer only on the subject concerning which they are talking, as on any subject, but with the women the percentage of those who answer on any subject exceeds the other. The percentage of those who talk in their sleep is much higher among those under twenty-five than among those above. In the ability to answer questions, those under twenty-five stand highest and those between twenty-five and thirty the lowest. The figures under *b* are not given for the different ages, as they show no special variation from the general percentage.

The existence of phenomena like the above has long been admitted by psychologists. Sir William Hamilton, to go no further back, in Lecture XVIII. of his *Lectures on Metaphysics*, collected and discussed a number of examples of them; and Carpenter, in his *Mental Physiology*, also devotes a chapter to the subject. These examples and discussions, unscientific though some of them were, led the way to further investigation, and are abundantly confirmed by later works on the subject. That there is a field of mental or cerebral activity which lies without the limits of consciousness, is regarded as an established fact. This paper has attempted to show the relative frequency of the phenomena involved in this "unconscious cerebration," and the results reached give an experimental verification of the psychological principles which others have maintained. The general sources of error in statistics of this nature are well known, and of course enter here to a greater or less extent. Some who answer evidently do not understand the questions and some are in doubt as to their answers. A number answer who are unable to give definite examples, but "think" they have noted such an experience. But there are, on the other hand, reasons which are sufficient to counterbalance these sources of error, and to give the results undoubted validity. First, there is in

most cases a large percentage of those who state that they have certainly experienced the phenomena under discussion, and a large proportion of these are able to give definite examples in confirmation of their statement. Again, there is often a considerable number who state definitely that they have had no such experience as the question indicates, and this, in a negative way, still further confirms the results.

A difficulty of considerable importance in the collection of statistics, is that of selecting a body of persons who will give average results. This difficulty has been felt to some degree in the present inquiry. The answers are from persons more or less accustomed to mental work and self-observation; nearly one-half of those who answer are under twenty-five years of age, while thirty-seven do not give their ages at all; and there are three times as many men as women. But the error arising from these facts cannot be very large, and may probably be disregarded, as the persons from whom these statistics come are in other respects a body well fitted for such purposes. They are largely college students and, moreover, not special individuals among the students, but entire classes, thus giving a body of persons selected perfectly at random, and likely to yield average results.

It is possible, then, in spite of the various sources of error, to form a conclusion regarding the general frequency of the various phenomena under discussion, as well as in the different sexes and in those of different ages. In general terms, it may be said that the answers of the women show that they have less experience with the results of unconscious cerebration than men. Possibly this is explained by less observation of such matters in their case. Concerning the different ages, there is usually, where the element of the mind's development and increase in maturity does not enter, a decrease with increasing age in the frequency of conscious results of unconscious action, which is frequently interrupted between twenty-five and thirty years of age. This interruption occurs so often that it scarcely seems due to accident or error, and yet no adequate explanation for it can be offered here. It is possible that the decrease with increasing age is due to the decreasing plasticity of the brain molecules or brain cells under normal conditions. Aside from the special results concerning each question, it does not seem that the figures warrant any further or more definite conclusions.

EXPERIMENTAL PSYCHOLOGY AT WELLESLEY COLLEGE.

MARY WHITON CALKINS.

After the discussion of the relative merits of experimental as compared with merely introspective psychology, a practical question suggests itself concerning the introduction of experimental psychology into the regular college curriculum. This is a complicated problem of expediency, the question of the equipment of the laboratory, of the relative amount of laboratory work, of the proper direction of students' experiments. Such questions are especially prominent in cases in which psychology is a required subject, and in which our course is a general one and must be adapted to students without especial scientific training or without particular interest in experimental work. In such a course, it is sometimes urged, the introduction of experimental methods burdens the general student with details valuable only to the specialist, substitutes technical minutiæ for psychological principle and tends to confuse psychology with the other sciences.

This paper is an attempt to meet difficulties of this sort by the record of a year's experience with a general course in psychology, making extensive use of experimental methods. In the fall of 1892 a course in "Psychology, including Experimental Psychology," was offered at Wellesley College as one of the alternative senior requirements in psychology. The course was taken by fifty-four students, of whom all but one or two had had no previous training in the subject. All of these had taken a year's course, including laboratory work, in chemistry, and only three had failed to follow a similar course in physics. Most had no training in physiology, and many of them had a more or less pronounced distaste for laboratory work. The aim throughout was to supplement, and in no sense to supersede, introspection; to lead students to observe in detail and to verify facts of their ordinary experience; to familiarize them with the results of modern investigation and with the usual experimental methods, and to introduce them to the important works of psychological literature.

The first month was devoted to a study of cerebral physiology. Ladd's "Elements of Psychology" was used in this early part of the course as a text-book. The class work included recitations, informal lectures and some written work on the part of the students. One of these papers, for example, required an enumeration, accompanying a rough diagram, of the parts of the human brain, as developed from the dorsal and ventral sides respectively of the three "primary bulbs." The study of the brain by text-book, by plates, and especially by models, preceded the dissection by each student of a lamb's brain. The brains had been preserved according to Dr. James's directions.[1] (Wide-mouthed candy jars, fitted with rubbers to prevent evaporation, proved an inexpensive substitute for the regular Whitehall and Taitum jars.) The dissection was under

[1] Since published in the Briefer Course in Psychology, pp. 81 90.

the general direction of the instructor. The students were provided with simple directions and were required to identify the most important parts of the brain. The results of this work were very satisfactory. The students, even those who had dreaded the dissection, were practically unanimous in regard to its value, as clearing up the difficult points in cerebral anatomy. In the class room, during this week, in which the dissection was going on, the principal theories of cerebral localization were discussed.

The next six weeks were spent in experimental study of sensation. About seventy experiments were performed by the students on sensations of contact, of pressure, of temperature, of taste, of hearing and of sight. The experiments, almost without exception, were selected from those suggested by Dr. E. C. Sanford in his "Laboratory Course in Psychology,"[1] but re-arranged with reference to the plan of the lectures and of the class discussion. Papyrographed descriptions of the experiments were distributed to the students and commented on in class before the experiments were undertaken. The instructor kept daily laboratory hours in order to answer questions and to offer assistance. Each student was responsible for the record of her own experiments.

In class, reports were made on the results of experiments, and recitations were conducted on the physiology of the different senses. The bearing of the different experiments on the theory of perception was carefully discussed. Special effort was made to free the word "sensation" from the vague, dualistic meaning which it often carries with it; sensation was treated as essentially "the first thing in the way of consciousness." The three theories of perception, Associationist, Intellectualist and Physiological-psychological, were carefully studied, and in this connection parts of Dr. James's chapters on "The Mind-Stuff Theory," "Sensations" and "Perception" were assigned for reading. Of course, in so elementary a course no new experimental results were gained. All the more important experiments usually performed were repeated. The taste experiments were so unpopular that I should never repeat them in a general class of students who are not specializing in the subject. I should also omit most experiments involving exact measurement. For instance, I should do no more than familiarize the class with the use of the Galton bar and of the perimeter.

Some of the students were genuinely interested in the experiments, carried them further than required and made independent observations; a large number, on the other hand, performed them conscientiously, but without especial enthusiasm; some cordially detested them from beginning to end; but almost all recognized their value as a stimulus to observation and as a basis for psychological theory.

The following questions, asked at an informal, forty-five minute examination, suggest the character of the experimental work:—

I. Describe fully the following experiments. State the theories on which they bear and the conclusions which you draw from them:—

a. The "colored shadows" experiment.

b. Scheiner's experiment.

II. What are the dermal senses?

III. What is the (so-called) joint sense? Describe an experiment proving its existence.

In the study of association, the old distinction between association "by contiguity" and that "by similarity" was replaced by one between "desistent association," in which no part of the earlier

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[1] American Journal of Psychology.

object of consciousness persists in consciousness, and "persistent association," in which all or part of it persists.[1] Dr. James's quantitative distinctions, corresponding with the terms "total," "partial" and "focalized," were also made. Students were referred to Hobbes, to Hartley, to Bain and to Dr. James, and were required to illustrate, by original examples or by quotation, the different sorts of association. This work proved very interesting and was valuable in co-ordinating psychological with literary study. The experimental work accompanying this study illustrated the value of association in shortening intellectual processes, and consisted simply in comparing the slower reading of one hundred unconnected monosyllables with the reading of one hundred connected words. Reading of passages of one hundred words in different languages was also carefully timed and compared.

A more extended experiment in association was later carried out. Each student wrote a list of thirty words, so associated that each suggested the next. The starting point was the word "book," suggested in writing, but not read until the time of the experiment. Each list was studied by its writer, who marked with a V the names of objects or events which were visualized; indicated with a C those connected with childhood life; classified the association, as desistent or as persistent (of quality or of object); and indicated, in each case, the so-called secondary law of the association (recency, frequency or vividness). Of course each list was written when the subject was alone and undisturbed.

The fifty papers thus prepared were carefully studied by one student as the foundation of her final essay. She compared with them similar ones obtained from two classes of school children of varying ages, and from a few people in middle life. Her results are interesting. One-half the objects or scenes (in the college lists, which were the only ones indicating this fact) were visualized. Only one-fifteenth related to childhood—suggesting that the predominance of Mr. Galton's childhood associations[2] is not universal. The lack of them may be due to the youth of the experimenters. A comparison of the lists shows that in the children's lists, recency is the most frequent explanation of suggestiveness, while in the college lists the vividness of the earlier object of consciousness is of greatest importance. The result tallies with the fact that "children's interests are more of yesterday and today."

The subject of attention was discussed on the basis of Dr. James's admirable chapter. The experimental work was in divided attention, the performance and accurate timing of two intellectual processes, first separately and then in combination.

A brief study of consciousness in its "identifying" and "discriminating" aspects was followed by a six-weeks' study of space-perception. Lectures were offered on the three chief theories, the Empiricist, the Nativist-Kantian and the Nativist-Sensational. The required reading included references to Berkeley, to Mill, to Spencer, to D. A. Spalding (MacMillan, February, 1873), to Preyer (Appendix C of Vol. II., The Mind of The Child), to James (parts of the Space-Chapter), to Kant (Æsthetic, "Metaphysical Deduction").

The experiments, of which there were more than thirty, illustrated the methods of gaining, or at least of developing, the space-consciousness. The theories of single vision were carefully studied and were illustrated by diagrams and by "Cyclopean eye" experi-

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1 Cf. an article in the Philosophical Review. July, 1892.
2 Inquiries into Human Faculty, pp. 191-203.

ments. The study of the perception of depth included an adaptation from Hering's experiment, in which the subject, looking through a tube, finds that he can correctly distinguish, within very small distances, whether a shot is dropped before or behind a black string, stretched before a white background. The fact and the laws of convergence were studied with the aid of a Wheatstone stereoscope.

There followed a consideration of illusions of space; and of Unvisual space, including the experiments suggested by Dr. James on so-called tympanum spatial-sensations, and others, with a telegraph-snapper, on the location of sounds. The scope of the work is suggested by the following outline for a paper required at the close of the work, and written without consultation of books or of notes.

SPACE.—Review Subjects.

(NOTE: Support all statements by description of experiments bearing on the question.)

I. Theories of Single Vision.
  a. Identical Point Theory.
    1. Statement.
    2. Limitations.
  b. Physiological Association Theory.
    1. Statement.
    2. Relation to *a*.

II. The Third Dimension.
  a. 1. The Nativistic Position (in every form).
     2. " Empiristic " " " "
  b. How do we gain (or at least develop) the consciousness of the third dimension?

(NOTE: Include reference to the questions: Can we perceive depth without motion of the eyeballs? If so, what are the means of perceiving depth in the given case?)

III. The Value of the Empiristic Theory to the Nativist.

In the study of memory and of the imagination, the only experiments were a few on "The Mental Span." Students were referred to James, to Burnham, to Lewes, to Ruskin and to Everett. Paramnesia was of course discussed.

Abnormal psychology received, throughout the course, comparatively little attention, because it seemed so evident that a careful study of the facts of normal consciousness must precede any scholarly consideration of the abnormal; because, also, there seemed special need of combating the popular notion which apparently regards psychology as a synonym for hypnotism and telepathy. The abnormal was therefore treated throughout from the point of view of the ordinary consciousness and its phenomena were discussed as exaggerated manifestations of the phases of all consciousness. The subject was naturally introduced by a study of dreams; hypnotism was the only other topic considered.

The study of the emotions and of the will was accompanied by no experimental work. Chapters of Höffding, of James, of Mill, of Spencer and of Darwin formed the required reading; James's theory of the emotions was discussed; a classification of the feelings, adapted from Mercier[1], but rejecting his physiological principle of division, was the starting-point of a somewhat practical discussion.

The last week of the course was occupied with reaction-time experiments, which had been postponed to this time, only through necessary delay in procuring the apparatus; the work should properly have been scattered through the year. There was time for

[1] Mind, Vol. IX.

little more than an illustration of method and an approximate verification of the more important results in reactions to sound and in more complicated reactions, involving association, discrimination and choice. Averages of simple reaction-times, with and then without signal, showing a general increase in the time of the latter, were made by several students and included in essays on attention. Students were required to read Jastrow's "Time-Relations of Mental Phenomena."

The study of volition led to several days' discussion of the problems of determinism and indeterminism. This was undertaken with the express remark that the subject is metaphysical and not psychological. The favorable result of this study confirms my opinion of the value of an occasional consideration of so-called metaphysical problems in a general course of psychology, with students who are neither studying philosophy nor specializing in psychology. To repress the philosophical questions, suggested by a study of psychological phenomena, is difficult; to consider them is harmful, only if the distinction between metaphysics and psychology is not sharply made; to discuss them may be of great pedagogical value. One may agree with Dr. James that "metaphysics fragmentary, irresponsible, and half-awake, and unconscious that she is metaphysical, spoils two good things when she injects herself into a natural science," but one may still believe that a course which properly is called psychology, because of the immense preponderance of psychological discussion and investigation, may yet make an occasional metaphysical digression. Such a plan at least avoids the difficulty which may be raised in the cases in which the course is a required one in the philosophical department—the objection that psychology, as a mere science among sciences, does not merit this pre-eminence.

In place of a final examination, a psychological essay was required. The subjects assigned were very general and were intended as subjects for study rather than as definite essay-headings. The immediate topic of the paper was to be decided after the study and not before. Such subjects as "Association," "Attention," "Memory," "Imagination," "The Psychology of Language," "The Psychology of Childhood," "The Psychology of Blindness," "Aphasia," "Animal Psychology," were chosen in this way. Writers on the first two subjects worked up the statistics, to which allusion has already been made. The specialization of topics, within the broad field indicated, showed itself in various ways. One student studied the prevalence among imaginative people of a "continued story," never written, seldom mentioned, but very significant, especially in the emotional life. Of fifty-four students who were questioned, nineteen have had such a story, whose characters grow, but retain a constant identity; whose situations change, but only in accordance with the plot, usually a simple one, of the story. With all the story began in childhood; with eight it still continues, but in all cases the interests of adult life have somewhat overshadowed it. A comparison was made with the experiences of thirty-six young men, students in Iowa College, with the result that only three testify to the possession, at any time, of a continued story.

The study of the psychology of blindness was accompanied by visits to the Perkins Institute. A student who writes on "The Imagination of the Blind," bases her conclusions on a personal study of twenty-five blind children. She questioned the children, consulted with their teachers, and read their compositions. Those who write on the psychology of childhood have made personal observations on babies and little children. One writes on "The

Senses of the Child," the other on "The Memory and Imagination of Chilldren." Both papers contain fresh material.

No topics in abnormal psychology were originally included, but several students especially interested in the subject write on "Hypnotism," "Dreams," "Illusions." Most of these kept written records of their dreams, during two months. Some interesting observations were made. One dreamer was able to reproduce, in a series of drawings, certain figures of a dream; several well-marked instances of reasoning are noted ; one writer makes the discovery that "my dreams are forgotten immediately upon waking, but that the instant I touch my head to the pillow the following night, the dreams of the preceding night come back with great clearness. In keeping a record, I was often obliged to wait until the following night to record something I had dreamed early in the morning." The analogy with the hypnotic memory is very interesting.[1]

In connection with the work of the course, a collection of statistics about colored hearing and number-forms was undertaken. Five hundred and forty-three persons, of whom five hundred and twenty-six were members of Wellesley College, were questioned. The general results are the following: Ninety-eight are affected in one or both of these ways; thirty-two have colored hearing, and seventy-eight have forms for numbers, for months, for days (or for all); fourteen have both colored hearing and some "form." These facts were gained by students, aided by a simple set of questions; in the cases of colored hearing, results were verified by questioning the subjects a second time after the lapse of two months; the number-form was in each case drawn by the subject; in all cases, records have been kept in uniform shape. Five essays were written on the basis of these statistics, of which each makes a special study of one or two cases of particular interest; from these essays I make occasional extracts in my brief report of our results.

Among the thirty-three cases of colored hearing are nineteen in which proper names of people suggest a color; nine instances of musical association; twelve cases in which names of days or of months are associated with color, and four of colored number-association, of which one is a colored number form. In seventeen cases letters suggest colors, usually by their sound, but sometimes by their appearance, and in three of these lists, the whole alphabet is included.

Galton's assertion that vowels more commonly than consonants suggest colors, is not confirmed by our results; among seventeen letter-associations, there are ten in which both consonants and vowels are suggestive; four in which consonants only are associated with color; and but three in which only vowel-associations occur.

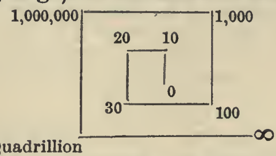
The manner of the word-coloring cannot be reduced to any general formulæ. It follows sometimes the color of the initial letter, sometimes that of a repeated letter. Sometimes, again, its color is that of the mixture of the colors of the different letters. In one case, "0 by itself is gray; when occurring with other numbers it takes the color of the accompanying number. Thus 80 is blue, the color of 8."

Like most observers, we have found that o and *i* correspond respectively with white and black — and this is almost certainly

[1]Cf. Ribot, Diseases of Personality, p. 118 (I know no other reference to this phenomenon).

through association with the appearance of the letter. The only positive exception which we find[1] in which *i* is red, falls in with the more indefinite generalization of MM. Beaumis and Binet[2], that either *i* or *a* is black, white or red. We find also the following instances of apparent uniformity: In six cases out of twelve, *a* is blue; in five out of twelve, *e* is yellow, and in six out of thirteen, *s* is yellow. This last is the only instance of uniformity among consonant-associations.

Our recorded number-forms, of which there are forty-eight, are of most varied sorts, including single and parallel lines; lines horizontal and vertical; zig-zags, curves and one Greek border,



One subject has "two forms, one for positive numbers to infinity, and another (extending in both directions from 0) including negatives, infinitesimals and fractions."

Not all changes of direction are at 12 or at 10. "Out of twenty-four, in which the lines are continuous,

Seven turn at 10
Six " " 12
Five " " 20
Five " " 6"

About two-thirds turn toward the right. Several forms are in tri-dimensional space, and we have one elaborate description of a form stretching away from the subject, in which the more prominent numbers stand out like mountain-peaks and hide the intervening ones.

Our records include several cases of pronounced emotional association combined with a sort of dramatization of numbers or of colors. Thus, one subject writes, "1, 2, 4, 7 and 8 are reliable, quiet, well-disposed, but not brilliant numbers; 3 is a sharp, shrewd, noisy and disagreeable number, always making as much trouble as possible. For 13 I always had a great antipathy. It had all the disagreeable qualities of 3 added to a pertness and aggressiveness which made it repugnant to all the other numbers, with which it seemed never to associate. I never wanted to be thirteen years old."

The circle is the most constant of the month-forms (occurring in eighteen out of fifty cases). Its plane is in most cases parallel, in one perpendicular, to the plane of the earth. In the forms for the days of the week, Sunday almost always occupies a conspicuous position. "When the plane changes, this day is higher than the rest, so that in passing from Saturday to Sunday, a step up is made." To one person "the first three days of the week seem much greater than the last three. In vacation, I always plan to make visits, receive company on the first three days of the week; the other three seem crowded together and insignificant."

No new explanations of the phenomena were offered or discussed. Only one affirmative answer, among twenty in the negative, was received to the question: "Can you compare your form with any

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[1] There is, besides, one case in which *i* is grey.
[2] Revue Philosophique, April, 1892.

pattern of wall-paper or carpet, with any crack in the plastering or with any other line which you might have seen when learning the numbers in childhood?"

So far, then, as our results support any theory, they tend toward one that is physiological; at least, they oppose, especially with reference to colored hearing, the explanations through any other than forgotten and fundamental childhood associations.

PSYCHOLOGICAL LITERATURE.

I.—ACTION AND VOLITION.

By Professor J. M. Baldwin.

Chauveau: *On the Sensori-Motor Nerve-Circuit of Muscles; Brain.* LIV. and LV., 1891, p. 145 ff.

This paper is an experimental attempt to establish a so-called Sensori-Motor Nerve-Circuit for the Muscles, i. e., a direct inmovation of muscle due to a current passing up from the muscle by the sensory branch and down by the motor branch of its nerve. For this inquiry Chauveau uses two muscles of which the sensory nerve courses are isolated from the motor, the sternomastoid (voluntary) and the muscles of æsophagus involved in deglutition (involuntary). In the case of sternomastoid, Chauveau finds, operating on the horse, (1) excitation of the motor branch gives contractions with no sign of sensibility; (2) excitation of the sensory branch gives contraction, also slightly delayed. Conclusion: A connection between the central ends of the sensory and motor courses, i. e., a reflex "circuit." The same result appears when the branches are respectively cut. Excitation of the end of the motor branch which adheres to the muscle gives contraction; excitation of the corresponding sensory branch end gives no result, and excitation of the end of the fixed motor branch gives no result, of that of the fixed sensory end gives contraction; so that the commotion always travels one way in the circuit, up the sensory and down the motor branch. The only differences in the stimulation of the muscle from the centripetal as against the centrifugal course are that the former is delayed in time and requires a stronger current. Testing the same muscle by simple section, i. e., by the withdrawal of physiological support rather than by positive electrical excitation, the results are the same, except in one particular. As would be expected, cutting the motor nerve gave paralysis of the sternomastoid, but cutting of the sensory branch had no effect upon the muscle. "Section of the sensory branch does not abolish movement," and "I failed to obtain evidence of any alteration of function." The same experiments on involuntary muscles—those of œsophageal deglutition—gave the same results, except that last named. Here section of the sensory branch produced "sometimes transitory and irregular paralysis, sometimes a peristaltic incoordination." Chauveau notes that the difference between voluntary and involuntary muscles in respect to the effect of the absence of sensory nerve support contradicts the results of Claude Bernard, who by cutting a series of lumbar sensory roots in the dog obtained motor disturbances in the corresponding hind limb. He explains the contradiction on the ground of differences in the relative complication of the muscle in question with others in performing coordinate functions. The motor branch in one case may be excit-

able through its connection with the sensory branch at the same time that it is not dependent upon the sensory reports of the latter for its inmovation: its connections with other sensory functions may still be sufficient. But in another case (Bernard's) it may be both excitable through and dependent upon the sensory reports.

It is evident that there is another possible explanation, i. e., that while sensory reports are sufficient for the regulation of involuntary movements, they are not sufficient for voluntary movement; these latter are regulated from a higher co-ordinating centre. This alternative Chauveau suggests and rejects, although he admits the physiological difference it requires, i. e., "one (muscle) is automatic, not influenced by the psycho-physiological centres, the other is a part of the voluntary locomotor system, influenced by these centres." The difficulty with his explanation is three-fold: (a) If the dependence of the motor function in question upon the support of the sensory branch be inversely as the degree of implication of other sensory connections, then the most complex and coordinated function, i. e., walking, would be most free from impairment when certain of these sensory connections are cut. Such relatively simple function as moving the head up and down upon the neck—due to the contraction of the sternomastoid—we would expect to be most impaired by cutting the sensory branch. (b) We may ask why such sensory support from other connections does not also avail to prevent impaired function in the case of the involuntary muscles? (c) Even though the kinæsthetic regulation by groups of sensory nerves be allowed, we would expect some impairment of function when the entire sensory contribution from the muscle in question is cut off; the amount of laming would be a matter of degree. But this is contradicted both by the result on the sternomastoid, and by Chauveau's experiments in cutting the four nerves supplying the toes of pigeons. He found that "complete sensory enervation of an extremity does not appreciably disturb these (the locomotor) functions," and "they roost indifferently upon the normal and upon the enervated foot." I may add, also, that these experiments upon the sternomastoid are valuable for the discussion of the regulation of voluntary movements, since all kinæsthetic support from "remote" sources (the eye, ear, etc.) are here ruled out.

Other interesting results are: Contractions of facial and lingual muscles by stimulating the nuclei, in the fourth ventrical, of the seventh and twelfth pairs; contraction of abdominal and spinal muscles by stimulating sensory cells of cord between the last dorsal and first lumbar root, after section of the cord from the brain (artificial respiration being carried on); co-ordinate respiratory movements in horses, after separation of the medulla from the encephalon, by stimulating the intercostal nerves. As to the mechanism of the sensori-motor circuit thus demonstrated, Chauveau criticises the ordinary kinæsthetic theory as being too complex, and adopts the view that there is a repropulsive wave which runs toward the muscle along the sensory course, reaching the muscle simultaneously with the motor impulse, and regulating the muscular contraction.

WALLER, *The Sense of Effort; An Objective Study; Brain.* LIV. and LV., 1891, p. 179 ff.

Dr. Waller attempts to ascertain by experiment the locus of the sensation of fatigue after muscular work and thereby also the locus of the sensation of muscular effort; holding that the former is related to the latter as an after-effect to a first-effect, analogous to

the relation between retinal images and after-images in vision. The main results on fatigue are as follows (he gives details of experimentation, apparatus, graphic records, etc., for which the article must be consulted): (1) The nerve courses do not contribute to fatigue. (2) There is no diminution in the lateral enlargement of a muscle from direct (artificial) stimulation after a series of maximum voluntary contractions, i. e., voluntary fatigue has not exhausted the muscle. (3) If a maximum series of voluntary effects be interpolated between two maximum series of direct effects, the second series of direct effects suffers no diminution, i. e., there has been recovery in the muscle for direct work during the series of voluntary effects. (4) The same recovery of the muscle for voluntary work during direct stimulation is seen by interpolating a series of direct effects between two series of voluntary effects. Waller agrees with Mosso, who obtained similar results, in holding that this recovery can only be in the peripheral organ in the former case and in the central organ in the latter case. (5) There is no diminution in voluntary effects after a maximum series of direct effects. (6) In the longitudinal effect there is a diminution both in direct maxima during voluntary work, and (less) in voluntary maxima during direct stimulation: a result which again agrees with that of Mosso. The general conclusion is that "voluntary fatigue depends more upon central than upon peripheral change." Further, the lateral (rigor) effect, in voluntary contraction of fatigued muscles slightly precedes and outlasts the longitudinal effect; this delay in the subsidence of the lateral effect is mainly due to a continued action of the centre upon the fatigued muscle, "a contraction reminder," "a residual discharge from the fatigued centre." Further, the cessation of voluntary longitudinal effect is quicker in fresh than in fatigued muscle. These results follow the use of a spring dynamometer rather than the weight-lifting method of Mosso. Waller criticises the latter on the ground that a weight is often lifted by more than the minimal effort really necessary to lift it. This criticism is proved by the fact that the weight method applied to the last mentioned experiment gave a longer persistence of the longitudinal than of the lateral effect, showing that some strain was relieved before the longitudinal effect actually produced by the weight was touched. The same criticism is proved to hold also *mutatis verbis* for the beginning of a weight-experiment, i. e., some force is spent before the weight begins to rise.

Waller also experiments on the flying-up of the arm when suddenly released from a voluntarily-lifted weight. He finds (using Douder's apparatus) that the upward spring of a fatigued is greater than that of a fresh arm. This agrees with Mosso's result that in a succession of such "releases" the amount of spring progressively increases. Waller explains the fact by the result given above that there is a "contraction reminder," which gives delay in the cessation of the nervous discharge in consequence of central fatigue. (This accounts both for the fact of such an upward spring and for the fact that it increases with fatigue; while Mosso's explanation, i. e., that greater stimulus is sent to the muscle in consequence of its fatigue, only accounts for the second fact.) A series of experiments is reported, further, on the estimation of weights when the muscles of the hand are "directly" stimulated. He found, in opposition to Ferrier, Goldscheider and Bernhardt (who, however, Waller claims has been misquoted in this connection), that under cathartic excitation of the digital flexors by the median nerve, 1000 gr. was barely distinguished from 3000 gr., or 500 gr. from 1500

gr., while, when voluntarily lifted, 1000 gr. and 1500 gr. were distinguished. In the case of faradic stimulation, practically no discrimination of weights was possible. This removes one of the strongest experimental arguments for the exclusively peripheral estimation of weights.

Ingenious as Waller's main argument is, it leaves two loop-holes of escape from his conclusion: (1) Granted that in fatigue the centre is mainly exhausted, it does not follow of necessity that this centre is the point of "incidence" of the feeling of fatigue. Mosso shows that central exhaustion has a chemical peripheral effect; this may occasion the feeling of fatigue. Or central expenditure may draw a kinæsthetic centre apart from itself, this latter contributing the feeling of fatigue. Waller's inference is a highly probable one, but not the only possible one. Further granted for the moment that the inference is correct, it only puts the sense of fatigue somewhere in the centre, not necessarily in the motor seat, unless we agree with Waller in denying the distinction between sensor and motor rests. (2) Granting the full value of the experiments, they again go no farther than to render probable the point of "incidence" of the sense of effort. Such a feeling may still arise in a kinæsthetic centre in dynamic connection with the working motor centre, or from "remote" peripheral courses. Nevertheless, logical alternatives aside, Waller's conclusion is undoubtedly the best interpretation of Waller's premises.

DELABARRE, *The Influence of Muscular States on Consciousness; Mind*. N. S. 3, July, '92.

This paper is largely a summary of the author's *Ueber Bewegungsempfindungen*, which will be noticed later. In this connection, only Delabarre's criticism of Waller may be referred to. Delabarre claims that Waller's argument contains three assumptions: (1) "That the objective signs of exhaustion are always indicative of a previous expenditure of energy in the same parts." [Not so. Waller uses the same muscle for voluntary and direct excitation. His assumption is: Granted both voluntary and direct work from the same muscle and objective exhaustion only for voluntary work, then the objective exhaustion must be in the nervous centre for this muscle. This is valid, and Delabarre's statement is a fallacy of conversion.] (2) "That a subjective sense of fatigue is indicative of a corresponding previous effort, and sense of effort, in the same parts." [Not so. Another involved conversion. Waller's position is: Granted a maximum voluntary effort and sense of effort directed to a part, and a sense of fatigue following the exercise of the same part, then the seat of this sense of exhaustion is the same as the seat of the sense of effort. This does not necessarily follow, as I have said above, but is physiologically extremely probable.] (3) "That objective signs of exhaustion are indicative of a subjective sense of fatigue, and objective signs of effort of a subjective sense of effort, localized in the same parts." [I also find this assumption, and have above expressed an opinion of the extent to which it invalidates Waller's conclusion.]

MÜNSTERBERG, *Mitbewegungen, in Beiträge zur experimentellen Psychologie*, Heft IV., 192.

Professor Münsterberg gives a résumé of the discussion on the question of the concurrent inmovation of symmetrical movements on the two sides of the body, against the old view that there was a direct tendency to such symmetrical movements when either side

is stimulated; he opposes both theoretical and experimental considerations. His experiments, executed in co-operation with several of his students, consisted in the voluntary performance of preconceived drawing and tracing movements (circles, triangles, squares, etc.), with one or both hands; then in occupying the attention with one hand only and observing the behavior of the other hand; or in withdrawing the attention altogether and observing the behavior of both hands, etc., through the various combinations available. His results lead him to the conclusion that in movements up and down and away from the body, such symmetrical movements do not occur; but that in movements right and left from the body such movements do occur. These latter cases he explains as due to the maintaining of the equilibrium of the body. His conclusion is that "inborn symmetrical co-ordinations of the muscles of the extremities do not exist." [This conclusion is by no means proved by M.'s interesting experiments. In the first place, the present writer finds it almost impossible to keep the attention so constant as not to interfere with the so-called "Mitbewegungen." Further, if Waller's result be true, there would be a certain residual discharge in the muscle after voluntary attention is withdrawn from it and this ought to give a certain amount of movement, symmetrical or asymmetrical, according to the voluntary movement. It is quite possible that there is such an element, but that it is drowned in the grosser tensions due to equilibrium, maintenance of balance, required habit, etc. Such a tendency could only be measured by a graphic record under conditions which ruled out the grosser sources of error; not by the rough explanation of a group of people standing or seated around a table. But, more than this, is experiment on adults likely to throw any light on this question at all? Everybody admits that our adult movements are massed in asymmetrical co-ordinations, which represent the strongest dynamic tendencies. There are, also, facts on the affirmative side of the question, such as "crossed reflexes" (see this Journal V. '92, p. 84). In the case of infants we have "crossed" responses in sleep (see my observations in Science XIX., 1892, p. 16, and Preyer's Mind of the Child, I., p. 207 ff). M.'s citation (p. 195) of my observations in connection with the development of right-handedness (resumed above) does not take account of the fact that in reporting these observations I added, a little later on, "In many cases the left hand followed slowly upon the lead of the right;" this was also true in the cases in which the left hand led—the right followed after in the same direction. A crucial test of the general question might be reached by Gotch and Horsley's new electrical method; the galvanometer showing to what extent, if at all, the cortical stimulation of a muscular group affects the motor nerves of the opposite side. As far as Gotch and Horsley's results on cats and monkeys bear on the question, they indicate that the tendency to bilateral performances is relative to the intensity of the stimulation —what we would expect from the general principle of diffusion. (Gotch and Horsley, "On the Mammalion Nervous System, etc.," Croonian Lecture, Phil. Trans., 1891.)]

SCHENCK, *Uber den Erschlaffungs Process des Muskels Pflüger's Archiv*, LII., 1892, p. 117.

S. asks the question: Why is it that the down-slope of the curve of contraction of a muscle exhausted by cooling is as steep as its up-slope, while the down-slope of that of a voluntarily exhausted muscle is not as steep as its up-slopes? He surmises that the less

abrupt down-slope of the voluntarily fatigued muscle is due to the greater consumption of its "reserve elements," apart from the production of lactic acid (I. Rauke). He asks the question, accordingly, whether the relaxation of a muscle in general takes place more slowly when its reserve elements are fewer. This he investigates by an experiment, the conditions of which are to compare the curve of a voluntarily fatigued muscle with that of a muscle whose excitability is reduced by lactic-acid artificially, i. e., in such a way as not to reduce the "reserve-elements" of the preparation. He injected the gastrocnemius of a frog with a .125% lactic acid solution. Its down-slope, like that of the cooled muscle, was less steep than that of the normally fatigued muscle. The same resulted with muscles injected with .1 to .2% soda. The rest of the paper is devoted to a discussion of theories of the molecular processes of muscular contraction and relaxation.

SCHENCK, *Ein apparat zur Verzeichnung von Länge und Spannung des Muskels*, Ibid, p. 108.

The title of this article indicates its contents.

WALLER, *On the Inhibition of Voluntary and Electrically Excited Muscular Contraction by Peripheral Excitation*, *Brain*, LVII., 1892, 35.

Waller asks whether the diminution in the force of a voluntary muscular contraction brought about by the superposition of direct electrical excitation is due to central inhibition (Fick), or to peripheral inhibition in the body of the muscle (Mosso), or to the excitation of the antagonists. Experimenting on the flexor muscles of the forearm and taking records, both by the dynamograph and "bag recorder" (see description in Brain, 1891, 206), he finds that there is a diminution in the maximum voluntary longitudinal effect, due to the electrical stimulation, but an increase in the maximum voluntary lateral effect. Further, that voluntary contraction superposed upon maximum direct foridization increases both the longitudinal and lateral effects, but that while the sense of maximum direct plus voluntary longitudinal effects is less than the maximum voluntary alone, the sense of the maximum direct plus voluntary lateral effects is more than the maximum of either taken alone. He explains these results largely by the stimulation of the antagonists (extensors); supporting this view by researches on the elevator muscles of the lower jaw, which have no antagonists (in which case the phenomenon in question does not appear), and on the flexors and extensors of the arm (which when both directly stimulated reproduce the phenomenon). A farther question is: Does the cessation of voluntary contraction involve simply a cessation of central voluntary emission, or a stimulation of the antagonists? Waller holds that the former is the main effect. He finds that in cases where the antagonist (extensor) is contracted, there is, on the release of the muscle (flexor), a prolongation of the lateral effect as compared with the longitudinal effect; but in voluntary release of the flexors, there is no such prolongation of the lateral effect. He also finds that in simultaneously grasping with one hand and letting go with the other, the two effects (curves) begin to appear simultaneously; but if the agent in each process be foridization, the muscle stimulated begins to contract before the released muscle begins to cease contracting. Turning from the effects of induced to those of galvanic currents in connection with voluntary contraction, he reaches conclusions which confirm Pflüger's and Waller and De Walleville's earlier results (Phil. Trans., Royal Soc., 1882). The general result is that "active arrest of action, i. e., true physiological inhibition of voluntary muscle, has not yet been demonstrated."

GENERAL MISCELLANEOUS.

EUGENIO TANZI. *Cenni ed esperimenti sulla psicologia dell 'udito. Rivista di Filosofia Scientifica.* December, 1891.

One-half of this article is a review of theories, physical, physiological, and psychological, on sensations of sound. The second half is an account of the author's experiments on the reaction-time for major and minor chords. The keys of a pianoforte, the reagent's key and the chronoscope were joined in one circuit. The results are shown in two tables, the one for simple reaction-time, the other for choice between reaction and no-reaction. Conclusion: The minor third has a "character of greater evidence" than the major, but is not always more quickly perceived. E. PACE.

SIEBECK, *Beiträge zur Eutstehungs-Geschichte der neueren Psychologie.* Giessen, 1891. S. 34.

This includes three studies on the growth of psychology during the perion between Aquinas and the Renaissance. The first is devoted to Eckhart, the other two to Buridan. As an advance upon his scholastic predecessor, E.'s chief importance lies in his treatment of the feeling side of mind. His recognition of an inmost soul-phase which blends subject and object, foreset and will in a higher unity is at once a departure from the old classification and a step towards the new. His mysticism asserts itself in the contest which he assigns to this unity, or "Fünklein," viz., the knowledge of God and of His indwelling in man. Still, his conception of the "Gemüthsleben" is an unmistakable prelude of modern teaching, and his insertion of the "Minne" between thought and will suggests quite plainly the tripartite division of mental phenomena.

Buridan's labors are marked by independence of thought and a preference for empirical methods. These traits are shown especially in his psychology, which is generally under-rated or minimized to the fable of the ass—whereof there is not a word in his writings. His doctrine of free-will is, in its earlier stage, an echo of the traditional "libertas oppositionis," the power to choose between opposites for which the motives are equally strong. But this appears later on simply as a means of attaining ethical freedom—the "libertas finalis ordinationis." To effect the transition, Buridan narrows freedom to a power of suspending judgment of the intellect upon its presentations, until, by further investigation, that real good be discovered, which claims the assent of the will.

More interesting for modern psychology are the "beginnings of a psychical mechanics" discernible in B. His attempt at a simpler classification of sense perceptions, based on genetic principles, breaks ground for the Associationists. In his answer to the question whether out of several simultaneous presentations each is as clearly perceived as though it appeared singly in consciousness, he takes into account the intensity and contrast of such presentations and the direction of the attention.

The "Relativity of Sensations" is one of his principles. Weber would have welcomed B.'s observation that not every slight increment of stimulus produces a corresponding increase of sensation; and Goethe might have found the germ of his color-theory in Buridan's treatment of light and shade. Add to this his careful analysis of feelings pleasurable and unpleasurable, and of their interaction under varying intensity, and it will be clear that B., whatever effect he may have had on his contemporaries, was not far from the line of thought which psychology follows to-day.

Ueber den Hautsinn. De phil. et med. Max Dessoir. Du Bois-Reymond's Archiv f. Phys. 1892. Heft III. and IV. pp. 177-339.

In a theoretical introduction of seventy pages, the author discusses the concepts of sensation and perception, the qualities of sensation, associated and after-sensations, the law of specific energy, the objectification of perceptions, and the classification of perceptions. The theory of specific energy in the sense "dass ein und derselbe Reiz diese verschiedenen Wahrnehmungen hervorbringen köune und dass eine Mehrheit von Reizclassen * * * ein und dieselbe Wahrnehmungsart erzenge" is held to be false. (P. 245.) In the restricted sense that: "Eo kommt einem jeden Sinnesapparate eine specifische Erregung, jedem grosshiruniede bezirke eine specifische Function zu" (p. 281), the principle is accepted. As regards the "objectification" of sensations, the author joins the group which holds that externalization is finitive, and the reference to an ego derived. (P. 224.)

The results of the long and often heroic experimental study of the temperature sense (p. 246-339) which must, in the outset, be recognized as very unequal in scientific worth, are categorically as follows:

The temperature sense is one as regards modality, heat and cold being the two qualities of one and the same sense. There are no hot and cold spots. There is but one kind of end apparatus, and this can be acted upon by only one kind of stimulus. Weak to moderate temperatures cause pleasurable feeling; extreme or intermittent ones, harmful feeling; continuous stimuli give sensation of fluctuating intensity. Of several successive stimuli, the first are felt moderately in comparison with following ones; but still later stimuli are felt less strongly again. D. assumes that the time which elapses between the feeling of heat or cold and the feeling of pain, when extreme temperatures are applied, is a measure of the intensity of the temperature sensation, and repeatedly makes use of this assumption in his proofs. Stimulation of a sensory nerve by heat, cold (263), pressure, or electricity (266) causes no sensations of heat or cold; or if the last-named causes such sensations, this is to be explained by vaso-motor modifications and not by the customary theory of specific energy. (P. 269.)

Probably the "free endings" in the lower layers of the epidermis are the end organs of the temperature sense, though this is not proved. (P. 280.) The lower part of the œsophagus, the stomach, the mucous membrane of the regio respiratioria, etc., are insensible to temperature stimuli, and afford means of determining the temperature end organs by comparative histological examination. (Pp. 275-280.) The Gyr. sigm. is the cerebral centre for temperature sensations. (P. 283.) In six pathological cases, the quality and degree of temperature sensations were recognized, but not their location (p. 284). There is no proof of distinct hot and cold nerves, and the "beautiful hypotheses" of different resistances in the different fibres are "built upon the sand." (P. 285.) Burn wounds are sensitive to temperature or not according as lower layers are present or not. (P. 287.) Long continued heat or cold reduces sensibility. Galvanic electricity does not affect it. Kal. brom. and chloral hydrate taken internally increase—cognac and caffeine first increase, then decrease temperature sensibility. External application of five per cent. solution of cocaini mur. to mucous membrane made the part anæsthetic for pressure and pain without affecting temperature sensibility; while a twenty per cent. solution applied to the skin produced no effect; .005 grm. of a ten per cent.

solution of the same, injected subcutaneously, produced anæsthesia for temperature, later for contact, touch, electricity, pain and rendered "a small elliptical outer zone" hypersensitive for all these stimuli. Similar results were obtained with .015 grm. morphini hydrochlor. (P. 288.)

Mustard plaster produces hyperalgesic and reduces temperature sensibility. The result of Adamkiewicz, that sensibility for touch and pain is increased at the place of stimulations by mustard plaster and decreased on the corresponding part of the other side of the body, and that no such bilateral transfer of effects occurs in the case of temperature sensibility, is not tested, but is rejected by the author.

The median line of the body is less sensitive than either side, the right side than the left. The dorsal has a lower threshold, but a less fine discrimination than the volar. Discriminative sensibility is greatest on the back of the upper arm, least in the middle of the back; is not much affected by the normal temperature of the skin; stands in no clear relation to the thickness of the skin; varies within smaller limits than the sensibility for pressure and distance in different parts of the body and does not have its maxima and minima at the same places as the pressure and space senses (p. 293). The points at which temperature sensations pass into pain are from +48.7° C. to +58.6° C. for heat and from +2.5° C. to +4.1° C. for cold, according to the duration of stimulus and the part of the body affected. (P. 294.) Temperature stimuli are recognized as distinct at from 1.2 mm. to 6 mm., according to part of body. Increase of the surface stimulated increases the intensity of the sensation. Discrimination is best between 27° and 32° C. After-images are removed by the opposite stimulus and reinforced by repetition of the same stimulus. Weak stimuli give intermittent after-images. The continuous after-image of a strong stimulus lasts from 316 to 715 σ; for high degrees of temperature, producing pain, 1513 to 1,889 σ. Long continued stimulation wearies; brief stimulation refines sensibility for the opposite stimulus. Successive contrast is stronger than simultaneous. Successive and simultaneous contrasts supplement each other. A more intense stimulus may be replaced by a less intense stimulus applied to a greater surface.

As between minimal temperature (p. 303) stimuli and minimal contact stimuli, one may be in doubt which has been applied, but this does not prove that touch and temperature sensations are homogeneous. Cold objects feel heavier than warm, one to two and five-tenths grams having been added to the warmer of two thalers to make their weights seem equal. Cooling a part of the skin increases its sensibility for pressure stimuli. (P. 306.) For the determination of reaction time, the author devised a modified form of the Siemens-Pflüyer Fall Hammer, a "Finger-Contact" and a "Reizapparat" permitting variation of the intensity of stimulus and surface touched. (Pp. 306-311.) The author cannot report whether he and his subjects used the sensory or muscular reaction, because none of the gentlemen made the distinction. "The true reaction stands in the middle of these artificially developed extremes. (P. 312.) Reaction time for touch decreases with the increase of the surface touched and varies for different parts of the body; the point of the index finger having the longest; the neck and left cheek, the shortest reaction time, of several places examined. (P. 316.) Reaction time for temperature varies with the part of the body and the extent of surface touched; 215 to 951 σ time elapses between the sensation of contact and the sensation of cold when a drop of water is permitted to fall on the point of pulsation of the left radial

artery, the time increasing with the rise of temperature. (—10° to +20° C.) (P. 318.) A "preparation time" of two seconds is most favorable. (P. 322.) The topography of the temperature sense as regards reaction time, as given by Goldscheider (p. 317), is correct. The temperature reaction times as given by Goldscheider, Tanzi and Herzen (350 σ—750 σ for heat, 250 σ—600 σ for cold), are agreed to, while the much shorter times found by Vintsbgan are discredited.

As to the processes in the nervous system accompanying temperature sensations, the author rejects Lotze's "Oscillations," the Fick-Wunderli hypothesis that temperature and other skin sensations arise from different combinations of the same elements, the Blix-Donaldson-Goldscheider theory of hot and cold spots, and the Hering theory of assimilation and dissimilation. Instead of these, he awaits the "patient labor of long years."

In attacking more than a dozen theories in the field of the temperature sense, many of which have strong support, Dr. Dessoir shows very praiseworthy courage. Not so praiseworthy is the slight consideration or denial of facts alleged by men whose work is considered of the first order, when those facts stand in the way of the author's theses. For example, to facts alleged by Herzen (p. 281), and again to facts alleged by Adamkiewicz (p. 289), the author makes little other reply than that he does not believe them; and his explanations of facts, fundamentally controverting his own positions, reported by Blix, Donaldson, Goldscheider, Du Bois-Reymond, Rittes, Vintochgan and others, seem to an unprejudiced critic sometimes trivial, some times mutually contradictory, certainly inadequate.

All this is very sure to bring back upon the author a polemic as severe as his own. He will be the less able to meet counter attacks because of the serious inequality of his own work. For example, upon the critical question whether electrical stimulation of the sensory nerve causes peripheral temperature sensations, the author throws out 1017 of 1200 experiments, without giving any reason except that only the 183 retained were free from error (p. 268).

In page 300, experiments are reported in which the observer's auditory reaction is eliminated from the gross times obtained. This the author does by subtracting from the gross times the average reaction time given in Jastrow's Time Relations. This is so inexcusably bad that one wonders that the author thought it worth while to eliminate the observer's reaction time at all.

Of less importance are the objections to the author's special reaction time studies. His claim that the error in his sensibolometer is constant, when no guarantee is given as to the force and rate of its application, must be doubted. For the same reason doubt is cast upon the tests made with that instrument to determine the influence upon reaction time of the amount of surface stimulated (p. 313) and of the part of the body stimulated (p. 314).

On the other hand, the undoubted merits of this long and patient research will be recognized by every competent reader. The facts reported, as also the polemic which the author has mixed up with the report of facts instead of separating therefrom as one could have wished, will surely lead to a more general and thorough study of the questions involved.

W. L. BRYAN.

DERCUM, F. X., M. D. Note on a Chinese Brain. Journ. of Nervous and Mental Disease (New York), New Series, Vol. XVII. (Sept., 1892), 691–695.

Professor Dercum here presents his notes of an examination of "the brain of an adult Chinaman of the coolie class," comparing it with the only other Chinese brains treated scientifically, viz.: one by Mills and Parker in 1886, three by Moriz Benedikt in 1887, and two by Professor Dercum himself in 1889. He says in conclusion, "These brains, owing probably to the unusual sinuosity of some of the fissures, together with the excessive transverse fissuration, have a physiognomy, as it were, of their own. They certainly, in general appearance, look different from the average white brain that we handle, and very different from the brain of the negro."

A. F. CHAMBERLAIN.

THE LABORATORY OF THE PSYCHOLOGICAL INSTITUTE AT THE UNIVERSITY OF GÖTTINGEN.

By William O. Krohn, Ph. D.

This laboratory is in many respects the best for research work in all Germany. It is peculiar in that it owes its excellent equipment to a liberal gift from a private individual, the state giving but a mere pittance to its support. To the generosity of a former student and friend is Professor Müller indebted for the laboratory of which any university in any land might be justly proud. Not only is the apparatus entirely new, but it is exceedingly well constructed. The rooms so recently set aside by the curator of the University for this laboratory are so well adapted to the purpose of research and of such generous size that the old time objection of "limited space" can no longer be urged against the Psychological Laboratory at Göttingen. Besides a very large auditorium, they have three other large rooms, well fitted for different lines of research work, and a well aranged dark room—indeed this dark room is an ideal one. With the new commodious quarters and their carefully selected equipment, Professor Müller and Dr. Schumann are well equipped for guiding a large number of students in experimental work. Professor Müller's investigations are well known and Dr. Schumann has recently distinguished himself by some important pieces of work. He is also a skillful mechanical contriver and every one of the old standard pieces of apparatus in this laboratory (e. g., the control hammer) has undergone some improvement. He is a very ambitious man, and most worthy of the best success. He certainly has a remarkable future. Like Müller, he aims at accuracy and thoroughness rather than the accomplishing of a large amount of poorly done work.

There is also an interesting historical point connected with the Göttingen Laboratory. It is this: Professor Müller is the successor of the renowned Lotze, and it was Lotze who, in his lectures and published works, gave such an impetus to experimental and physiological psychology. How fitting, then, that there should be such a well equipped laboratory at Göttingen, realizing in a sense the long cherished hopes of the man who stood peerless among his contemporaries in the demand for thorough-going investigation and the application of the scientific method to all classes of facts.

The following is a descriptive list of the chief pieces of apparatus in the laboratory, arranged according to the kinds of experiment to which they are devoted.

I. *Psychometric and Reaction Time Researches.*

Hipp chronoscope (new pattern) with control hammer, the latter made by Krille. The whole construction embraces many new and ingenious improvements; also a device for the electro-magnetic

tuning fork with double slides and a micrometer screw for adjusting the same. Cost 140 marks.

Electro-Magnetic Signal Hammer. The head of the hammer is kept in equilibrium, not by means of the usual coiled-spring, but by means of a weight so arranged as to drop on a felt pad. This arrangement insures steadiness of the hammer and excludes all noise except the signal. Cost 25 marks.

Sprechcontactapparat (Schalltrichter) with the electro-magnetic contacts necessary to its best use. Cost complete 105 marks.

Ewald's double contact key with attachments, 31.50 marks.

Electrical keys and contacts ; two compound contacts (five fold), simple keys and contacts, commentators, etc.

II. *Apparatus for Experiments upon the Time-sense (Zeitsinn)*, including platinum contacts and aluminium wheels. Pneumograph with tambour and attachments. Divers contact circles, a brass circular scale marked off into degrees, minutes and seconds, telephone statives, etc.

For other important apparatus in this line of research, see Schumann's exhaustive study in a recent number of the *Zeitschrift für Psychologie.*

III. *Researches upon the Memory.*

An especially constructed rotation apparatus, very complex in its arrangement.

IV. *For Psycho-physical Investigations.*

A set of eight weight holders, nach Fechner, together with weights (sheets of lead properly cut) belonging thereto. These are constructed after the manner of the originals used by Fechner himself in his experiments with lifted weights.

An arrangement (especially constructed and very ingenious) with aluminium levers for the purpose of recording the curves in experiments with lifted weights.

Apparatus for use in "Fühlraum" investigations, including an arrangement whereby the judgments are recorded in the form of curves drawn upon smoked paper.

Simple "Temperatursinnmesser" Warmtaster [Mischer].

Rotation discs with electrical apparatus for recording the clicks. A large "Rotationapparat" with five adjacent color discs, nach Aubert (Cf. Müller, Psycho-Physik). Licht-emdruck nach Exner (Wiener Ber. Nat. Hist., 2 Abtheilung 1868 Band 58 s. 60 ff.) for investigations upon the harmonizing of visual sensations; 280 marks.

An apparatus for fixing the head and holding it firmly, nach Helmholz. Darkened boxes used in the researches which have to do with light and color, Charpentier's "Kugeln," resonators, etc.

V. *Researches Along Various other Lines.*

Tonometer nach Mono, 180 marks. Spannungsmesser nach Bernstein, 125 marks. Kymographion with continuous paper.

Inducktorium, nach Du Bois-Reymond. Electrodes with clasps for the arms. Electrodes with arm bands, and various other electrodes.

VI. *Miscellaneous Apparatus.*

Resistance boxes (Siemens). Galvanometers with spring, nach Kohlrausch. Tuning fork interrupter, nach Helmholz, with spiral contact. Mariottes bottles and attacnments. Water motor. Metronome Rheocord, a fine balance, and various other pieces of minor

importance. This by no means includes the complete equipment. All of the older apparatus which Müller used in the larger number of experiments referred to in his "Psycho-Physik," and in his more recently published magazine articles, are included and should not be forgotten, especially those concerned with his experiments upon sensations of movement and the muscle sense.

UNIVERSITY COLLEGE, CARDIFF.
Oct. 12, 1892.

My Dear Sir:

The plan of philosophical teaching in this college is to a large extent determined by the requirement of the University of London, which most of the students look to for degrees. The same is true of most of the provincial colleges of England and Wales as well as of University College, London. In our case, however, the advent of a University of Wales will—when it comes—no doubt introduce some changes into the curriculum; especially, it is to be hoped, in the direction of requiring more thorough knowledge of some special department of philosophy from candidates for the higher or M. A. degree.

As matters are arranged at present, I have courses of lectures on psychology, logic and ethics continuing throughout the session of thirty-two weeks. Lectures on psychology are given three times, on logic twice and on ethics once a week. In this way an outline of the three sciences is given; the London syllabus for the B. A. and B. Sc. degrees being kept in view and, to a large extent, followed. Some weeks at the end of the psychology course are, however, commonly saved for the purpose of introducing students to some of the philosophical questions which arise out of psychological questions. In the treatment of psychology, emphasis is laid upon the continuity of mental life. The physiological material admitted is comparatively small in amount; and it is only introduced in so far as it clearly helps to psychological conclusions. In this respect the method approaches what Professor Bain would call "ascetic." So far as my experience goes the method is fitted both to stimulate interest and to guard against the confusion of mental with material facts in which beginners are so apt to get entangled. In the class of ethics, a considerable portion of the time is occupied in explaining and commenting on one or two leading works by English moralists, such as Butler's Sermons on Human Nature and Mills' Utilitarianism.

In addition to these classes, advanced courses of lectures are given on the history of philosophy, with reference to special periods and books which change from year to year, and on the more difficult questions of logic, psychology and ethics, as well as on political philosophy. Both in the ordinary and in the advanced classes, the instruction by means of lectures is supplemented by conversation in the class-room by means of written papers.

I am, my dear sir,
Yours faithfully,
W. R. SORLEY.

Prof. G. Stanley Hall.

THE AMERICAN

JOURNAL OF PSYCHOLOGY

VOL. V | APRIL, 1893. | NO. 3

ON ERRORS OF OBSERVATION.[1]

BY PROFESSOR JAMES MCKEEN CATTELL,

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Currents of thought often arise at different sources, and flow on for a long way before they mingle. This has been the case with the investigation of errors of observation in physics and in psychology. On the one hand methods for securing the nearest approximation to the true value from discordant observations have been studied by many of the most eminent mathematicians and physicists since the revival of learning. On the other hand the accuracy with which the external world is perceived has always been a central subject in psychology, and in the development of experimental psychology no portion has received more attention than the perception and comparison of differences in intensity. It has, however, to a considerable extent been overlooked that physics and psychology are concerned with the same phenomena. This is not surprising, as the points of view of the two sciences are different. Physics seeks to eliminate errors of observation; psychology seeks to study their nature. But the time has now come when each science should profit from the progress of the other. Physical science can better eliminate errors of observation by learning what is known of their cause and nature. Psychology will gain greatly in clearness and accuracy by using the methods of physics and mathematics.

The errors of observation with which physics and mathe-

[1] Read at the meeting of the American Psychological Association, Philadelphia, 1892.

matics have dealt are variable errors, such errors as would occur were each error composed of a very large number of comparatively small and independent errors, equally likely to be positive or negative. In this case the average of the observations is the most likely value, and its approximation to the true value is measured by the dispersion of the errors, and increases as the square root of the number of observations. In two important respects the mathematical theory needs to be supplemented by psychological experiment. In the first place, constant errors are entirely beyond the range of the method of least squares, and yet these are evidently more dangerous in physical observations than variable errors. Thus, for example, in the case of the personal equation of the astronomers, the variable error of an observer can be reduced to any desired extent by increasing the number of observations. But it was found on comparing the observations of different observers that they had constant errors far more serious than their variable errors. It was (and apparently is still) thought that the constant error of an observer becomes a variable error when the observations of several observers are combined. It is very unlikely that this is the case. The uniformity of the processes of perception and movement is greater than their variability. We may feel confident that the combined personal equations of all the astronomers would be subject to a constant error which cannot be eliminated by physical or mathematical science. But such constant errors depend on fixed psycho-physical conditions, and can be measured by the psychologist.

In the second place it may be urged that the theory of probability can only give a rough and ready account of the distribution even of variable errors. In measuring an inch an error of a mile will not occur, and a negative error of a mile is inconceivable. The probability assigned to such errors by theory is, indeed, extremely small, but the same probability is assigned to positive and negative errors, and they are not equally likely. It would seem that as a rule positive errors are more likely than negative errors. In measuring an actual inch, a positive error of two inches might occur, a negative error of the same size cannot occur. In ordinary errors of observation a corresponding preponderance of positive errors may be expected, and a correction for such excess must be empirically determined. The same holds for the averages which are so widely used in statistics. Thus, if the average weight of men be 150 pounds, men weighing 300 pounds occur, men weighing 0 do not occur. The average is not identical with the median, as required by the theory of probability. The assumption made by the mathematicians,

that an error is composed of a very large number of comparatively small and independent errors, cannot be admitted by the psychologist. If the fiction of indefinitely small errors be accepted at all, the elemental errors cannot be regarded as independent, but are interdependent and occur in groups. The distribution of errors will not follow simple and universal formulæ, but the greater our knowledge the more complicated will the formulæ become, and they will be as numerous as there are observers and observations. The deductions of Laplace and Gauss are of the greatest importance, but it should not be forgotten that the laws of nature cannot be invented, they must be discovered. It is within the province of psychology to supply physics with the formulæ it requires for eliminating errors of observation in special cases.

Turning now to what psychology can learn from physics, we find that the variable error of the method of average error and the probable error (or h as used in Germany) of the method of right and wrong cases are the error of observation of physical science. We may ask, why should there be an error of observation? Why should not the same stimulus be accompanied by the same sensation? The natural answer is that the conditions do not remain the same. In the first place the stimulus itself cannot be kept exactly constant. Lights are always variable, and sounds and touches cannot be exactly reproduced. Temperatures and smells are especially inconstant. Weights may remain nearly the same, but the manner of lifting them is always different. We have, therefore, a variable stimulus which in part accounts for the variation in sensation. In the second place the nervous mechanism is constantly changing. The sense organ is rhythmically exhausted and restored, and is subject to various irregular alterations. The nerves and paths of conduction in the brain would transmit more or less of the energy of the stimulus according to their ever changing condition. Lastly, the brain centres immediately concerned with perception alter greatly in metabolism. These latter changes are best known to us on the side of consciousness; there is a more or less regular rhythm in attention, and very numerous irregularities due to fatigue, interest, inhibition, etc. These sources of variation will sufficiently account for the fact that the same sensation does not recur. They are, indeed, so numerous and to a certain extent so independent, that they justify roughly the assumption of the mathematician, and the results of experiments show that the errors are in a general way distributed as required by the theory of probability.

In psycho-physical experiment two magnitudes are perceived and compared. The combined error of perception

would be larger than a single error of perception, being the square root of the sum of the squares of the separate errors, or nearly the error in a single case multiplied by the square root of two. We have further the errors of memory and comparison. The analysis of these factors at the present time would be very difficult, but I believe they would simply increase the variable error of observation, and introduce additional constant errors. This is not the view taken by Fechner, Müller, Wundt and others, to whom we chiefly owe the development of psycho-physical research and theory. They maintain that there is a threshold of difference, and when sensations differ by less than this amount there is no difference in consciousness, Fechner does not question the application of the probability integral to the comparison of magnitudes,[1] on the contrary it was he who first applied it to the method of right and wrong cases. He argues that a difference in the stimuli smaller than the threshold might be made apparent in consciousness by the error of observation, and would give the preponderance of right cases required by theory. But in about one-seventh of his trials he was doubtful as to which of the weights used by him was the heavier, and holds that in these cases the difference in the weights and the error of observation combined fell within the threshold, and that there was no difference in consciousness.

Prof. Fullerton and the writer[2] made experiments with lifted weights similar to Fechner's. In one series of 3000 experiments in which the probable error was much the same as Fechner's, the observers were doubtful 23% of the time, but on guessing which of the weights was the heavier they were right 62½% of the time. This is the percentage of right cases required by the theory of probability, on the supposition that the differences in consciousness follow Gauss' formula, and we may conclude that the difference in consciousness always exists and affects the course of mental life, even when it is so small that it cannot be detected.

Another case in which German psychologists have run counter to the theory of probability is in the assumption of a just noticeable difference. According to the theory of probability the apparent difference in sensation and the probability of correct judgment tend to increase continuously as the difference between the stimuli is made greater, but it is

[1]As implied by Peirce and Jastrow in their important paper (*On Small Differences in Sensation ;* National Academy of Sciences, III. [1884]), which for the first time denied the supposed fact of the threshold.

[2] *On the Perception of Small Differences;* Univ. of Penn. Press, 1892. The present paper is largely based on this monograph.

entirely arbitrary to choose one difference and call it just noticeable. A difference in the stimuli can be found which will be obscured by the error of observation 1 time in 10, or 1 time in 1000, but no difference can be called just noticeable, meaning that it and larger differences will be correctly distinguished, while smaller differences will be indistinguishable. In actual experiments Prof. Fullerton and the writer found that the difference fixed on by the same observer under changed conditions as just noticeable was not at all proportional to the error of observation, and with different observers the difference which they considered just noticeable in no way measured their accuracy of discrimination. In the many researches in which the method of just noticeable difference has been used, the just noticeable difference fixed on by the observer has probably been determined partly by his general knowledge of his error of observation (the difference he would seldom mistake) and partly by association, he choosing an apparently equal difference.

The last application of the theory of probability which I wish to make concerns the relation of the error of observation to the magnitude of the stimulus. The algebraic sum of a number of variable errors tends to increase as the square root of the number. In measuring the base line of a survey the variable error of observation increases as the square root of the length of the line. It seems to me the same relation might be expected to hold in a general way when the length of a line is estimated by the eye or compared with another line. Or to take another example, if we estimate one second of time and repeat the trial four times, the algebraic sum of the four variable errors, or the combined error in estimating the four seconds, will tend to be twice as great as the error in estimating a single second. If we estimate or compare the four seconds continuously, the same elements would to a considerable extent be present, and we might expect an error twice as great as in estimating a single second—not four times as great as required by Weber's law.[1] The error in estimating each of the several seconds might and doubtless would be different, and in the case of intensive magnitudes equal objective increments would seldom or never be accompanied by equal changes in consciousness, nor be subject to equal and independent errors. The theory of probability only considers the simplest and most general case. We must use all the knowledge we have

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[1] Constant errors increase in direct ratio to the magnitude, and would tend to follow Weber's law. But, curiously enough, constant errors have not been supposed by the psychologists to follow Weber's law. As a matter of fact "constant errors" are very inconstant and difficult to investigate.

as well as our theory, and the general formula must be adjusted to each special case.

In attempting to pull a dynamometer twice with the same force we do not compare the movements as we proceed, but the final result, and if the force were near the limit of our strength, the error might be less than for a smaller magnitude. We should expect a post-office clerk to judge very light weights better than a blacksmith, a blacksmith to judge heavy weights the better. We should expect to discriminate lights best within the range of ordinary daylight, and sounds best within the range of the human voice. Such results would be contrary to Weber's law, but are simply factors additional to the summation of errors required by the theory of probability. The relation between the error of observation and the magnitude of the stimulus will differ for each stimulus and for each observer, and will not remain constant even for the same stimulus and the same observer. But the usual increase of the error of observation with the magnitude of the stimulus is accounted for in a satisfactory manner by the summation of errors, and I should substitute for Weber's law the following: *The error of observation tends to increase as the square root of the magnitude, the increase being subject to variation whose amount and cause must be determined for each special case.*[1]

It may be asked if this view be correct, why do the results of researches confirm Weber's law? As a matter of fact Weber's law has not yet been confirmed exactly by any careful research, the error of observation usually becoming larger as the magnitude of the stimulus is taken larger, but almost always more slowly than in direct proportion to the magnitude. The attempt has been made by Fechner, Wundt, Helmholz and others to explain away the variations by additional hypotheses, but it is universally admitted that the validity of a law or hypothesis decreases as the number of subsidiary hypotheses increase.

I venture to think that it is an open question whether in the researches hitherto made the error of observation increases more nearly as the magnitude or as the square root of the magnitude. Researches in which the method of just noticeable difference has been used do not of necessity measure the

[1] Prof. Fullerton pointed out at the meeting of the association that the conditions which made the first fractional or elemental error positive or negative might make the following error tend in the same direction. So far as such a tendency is present the error of observation would increase more rapidly than the square root of the stimulus, and more nearly in direct proportion to it (Weber's law).

error of observation at all. The variation in adjusting the just noticeable difference would roughly measure the error of observation, but this has been neglected. All the researches on lights with which I am acquainted[1] (excepting that by Prof. Fullerton and the writer) used the method of just noticeable (or more than noticeable) difference. Now it is natural enough (considering its elasticity) to make the just noticeable difference within the range of ordinary daylight proportional to the intensity of the light. We see the same objects more or less brightly illuminated, and should tend to regard the differences in shade and color as equal differences, whatever the intensity. It may also be remarked that the mechanism of the eye (accommodation of the pupil and sensitiveness of the retina) tends to obliterate objective differences in brightness. Further in all these researches on lights (excepting Merkel's) the lights were side by side, and the time of exposure was not limited. In such a case the error of observation becomes much obscured, and almost any result can be obtained.

I venture to maintain this conclusion even against the very careful research by König and Brodhun, which supports Weber's law for a considerable range of intensity. It is especially difficult to adjust a just noticeable difference when the areas of light are very small, and for colors not usually seen. König and Brodhun found the just noticeable difference for different colors of apparently the same intensity to be the same (ca. $\frac{1}{75}$ of the light). Previously with much the same methods Lamansky found the just noticeable difference for red $\frac{1}{70}$, for yellow and green $\frac{1}{286}$, for violet $\frac{1}{106}$, whereas Dobrowolsky found for red $\frac{1}{14}$, yellow $\frac{1}{46}$, green $\frac{1}{59}$, violet $\frac{1}{268}$-$\frac{1}{67}$. The three researches were carried out in Helmholz' laboratory, and we may well be at a loss to draw any conclusions from such discordant results.[2] Perhaps the two best researches with lights have been carried out by Aubert and by Müller-Lyer. Both of these writers think their results do not support Weber's law.

It is not necessary in this place further to review and compare results of researches on lights and other stimuli. If it be admitted that the just noticeable difference be not proportional to the error of observation, the amount of work to be

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[1] Bouguer, Lambert, Arago, Masson, Fechner, Volkmann, Aubert, Helmholz, Plateau, Delbœuf, Kraepelin, Dobrowolsky, Lamensky, Breton, Ebbinghaus, Merkel, Lehmann, Neiglick, Schirmer, Müller-Lyer, König and Brodhun.

[2] More especially as Helmholz, in the revision of his *Physiologische Optik*, does not even mention Lamensky and Dobrowolsky. Nor does he refer to work not done in Berlin.

considered would be greatly reduced. Further, many researches by the method of average error and right and wrong cases have only a tolerable validity (*e. g.*, Fechner's and Merkel's) because the observer knew the relation of the stimuli before comparing them. In other cases (*e. g.*, with tastes, temperatures, touches and sounds), the stimuli have not been measured in a satisfactory manner. I believe the various researches are so disparate, having been made by so many observers (often young men working for a degree) and by such varying (and in many cases inadequate) methods, that the only general conclusion which can be drawn is that the error of observation tends to increase as the stimulus is made larger and usually more slowly than in direct proportion to the stimulus.

Before concluding I wish to notice the relation between the error of observation and the estimation of mental intensity. It has commonly been assumed that the variable error and the probable error (or h in Germany) are proportional to the just noticeable difference. The just noticeable difference has further been used to measure the intensity of sensation. The just noticeable difference is thus used ambiguously, on the one hand as a difference equally likely to be correctly perceived, on the other hand as a difference accompanied by an apparently equal increment in sensation. I entirely question the application of the error of observation to the measurement of the intensity of sensation. Supposing the intensity of sensation to be measurable, it may increase as the stimulus or (conceivably) as the logarithm of the stimulus, while the error of observation may be any other function of the stimulus[1] When it is evident that the error of observation may be increased or decreased in many ways without greatly altering the apparent intensity of sensation, I cannot understand how it has come to be used as a unit suitable for measuring the intensity of sensation. The error of observation is a physical quantity, a function of the intensity, area, duration, etc., of the stimulus, of the condition of the nervous system, and of the faculties, training, attention, etc., of the observer. That it should increase with the magnitude of the stimulus, and tend to increase as the square root of the magnitude, seems to me a natural consequence of the summation of errors. But I see no necessary connection between the supposed fact that the error of observation increases in direct proportion to the stimulus and the consequence which has been drawn from it that the intensity of sensation increases as the logarithm of the stimulus.

[1] This was noticed by G. E. Müller in 1879 (Zur Grundlegung der Psychophysic, p. 79-80).

The measurement of the intensity of sensation is not out of the question because the error of observation cannot be used as a unit. The attempt is made to accomplish this when for different intensities sensations are adjusted midway between two others, when they are made apparently half or double others, or, lastly, when they are made just greater or less than others in the sense that the difference in sensation is apparently equal. The question here is whether we do in fact judge differences in the intensity of sensations, or whether we merely judge differences in the stimuli determined by association with their known objective relations. I am inclined to think that the latter is the case. I find it comparatively easy to adjust one time, length of line or weight midway between two others, much more difficult to judge when one light or sound is midway between two others, and almost impossible to judge one temperature or pain midway between two others. The difficulty of making a decision increases as the objective relations are less familiar, and I believe that my adjustment is always determined by association with the known quantitive relations of the physical world. With lights and sounds, association might lead us to consider relative differences as equal differences, and the data would be obtained from which the logarithmic relation between stimulus and sensation has been deduced. With the force, extent and time of movement, Prof. Fullerton and the writer have shown that our estimates tend to follow the objective relations. But, in any case, if we merely judge the relations of objective magnitudes by association, we have no basis whatever for determining a relation between physical energy and mental intensity.

I conclude, consequently, that we cannot measure the intensity of sensation and its relation to the energy of the stimulus either by determining the error of observation or by estimating amounts of difference. The most natural assumption would seem to be that the intensity of sensation increases directly as the energy of the brain changes correlated with it. The relation between the energy of the brain changes and the physical stimulus is a physiological question. This conclusion does not mean, however, that psycho-physical research is valueless. On the contrary it is an important contribution to the science of psychology, whence its application will be extended to physical science, to art, to medicine, to pedagogy and in other directions.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CLARK UNIVERSITY.

EDMUND C. SANFORD, PH. D., *Director.*

I.

INTRODUCTION.

In beginning the publication of this series of minor laboratory studies, a word of explanation as to their nature seems appropriate. The aim of the laboratory in Clark University is original research, not only for itself, but as a pedagogical means. Many students, however, come to us from institutions in which psychological laboratories have not as yet been founded, and sometimes with no training in experimental science at all, and are therefore more or less unfitted to enter at once upon research work. For this reason the first year in the laboratory is devoted largely to practice experiments, such as those collected in the Laboratory Course already published in part in this *Journal*; but an essential part of each student's work during the year is the execution of some small and definite piece of experimental research, assigned by the director of the laboratory and worked out, after some months of general laboratory practice, under his supervision and generally with his active assistance at the start. The first six of the following papers give the results of such studies. The greater number of papers published under the title of "Minor Studies" will be of this character, but others will also be included (as in this instance that on the Pendulum Chronograph) when their length and subject matter are fitting.

ON THE DISCRIMINATION OF GROUPS OF RAPID CLICKS.

BY THADDEUS L. BOLTON.

With reference to groups of rapid clicks several questions may be asked, among others the following: 1. Is the rate at which the clicks of the group fuse into a continuous tone or noise dependent upon the number of clicks in the group, so

that a small group can be recognized as composed of discrete clicks when a larger group cannot?[1] 2. In the perception of a group that does not fuse, is there a tendency for any of the clicks to drop out, as Hall and Jastrow found that they tended to do when counting was added to simple perception? 3. Can the presence of one more or one less click be recognized when successive groups of different number of clicks are given, and under what conditions as to rate and number? The experiments about to be described have immediate bearing on the last of these, but throw more or less light also on the first and second.

Apparatus and Methods. In such experiments as these, the great difficulty is to get an instrument that will produce a sound of the greatest possible clearness and definition in controllable number and at regular intervals. The required sound was found in the click of the armature of a time-marker of the pattern furnished by the Cambridge Scientific Instrument Co. This sound was produced at the closing of the circuit; the sound at the break was avoided by making the armature react against a piece of soft India rubber. Though the avoidance of this break sound was not absolutely complete, it was not sufficiently loud to interfere with that at the make, and in general was wholly unnoticed. All that was required now to get a regular succession of uniform clicks was to make and break the circuit at regular intervals. This was accomplished by causing a heavy pendulum to draw a projecting platinum point across a series of equal brass plates separated by pieces of hard rubber of uniform thickness. Each plate was connected by a wire with a switch-board, which made it possible to add or subtract one or more of the clicks by throwing one or more of the plates into or out of connection with the time-marker. What was needed for the experiment, however, was not one series of clicks, but two separated by a short interval. To accomplish this, a second platinum point was attached to the pendulum at a suitable distance behind the other. From these platinum points a wire ran up the pendulum bar into a mercury cup opposite its centre of oscillation, thence to the battery (12 Leclanché cells), to the time-marker, and to the switch-board, thus completing the circuit. The pendulum used was like that devised by Drs. Bowditch and Warren, of which a full description may be found in the *Journal of Physiology*.[2] The especial value of such a pendulum for this work lies in the fact that its

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[1] Suggested, but not worked out to any extent, by Hall and Jastrow. *Mind*, XI. 1886, 58.

[2] Vol. IX. page 29.

velocity in the different parts of its arc varies less than that of ordinary pendulums. To prevent the friction of the platinum points from affecting the rate of the pendulum, and also to insure a better contact, they were attached to brass wire springs. The pendulum swung each time from a fixed position, where it was held till the required instant by a magnet. To prevent the points making contact at the backward swing of the pendulum, the set of brass plates was attached to a board which was hinged at the bottom and could be withdrawn when the pendulum swung backward. When this board was pressed forward so that the brass plates were in a position to be touched by the platinum points, it broke the circuit through the magnet just mentioned and released the pendulum.

In order to time the apparatus the time-marker was made to write upon a drum along with a Deprez signal driven by a tuning-fork of 100 vibrations per second. At the first setting up of the apparatus ten clicks were used and occupied in all .11 sec. They were consequently separated from one another by intervals of .011 sec. The apparatus was timed very frequently to ensure constant rate. In the later experiments the pendulum was made to swing through a greater arc. This decreased the interval between the clicks to .0065 sec., and in the last experiments the interval was made .0075 sec. The interval between the two groups of clicks was about .25 sec. This interval varied slightly in different portions of the experiments. The amount of variation in the velocity of the pendulum during the time it was passing the plates could not be detected in the tuning-fork record. In all the experiments the time-marker, besides giving the clicks, inscribed its motions on a smoked drum, by means of which an objective record was kept of what had actually happened, and any failure in the instrument was instantly detected.

In making experiments the operator was seated before the pendulum, managed the switch-board with one hand and brought the brass plates into position with the other, while the subject sat near the time-marker and in such a position as not to see the essential manipulations of the operator. The operator told his subject that there would be a certain number of clicks in the first group, *e. g.*, four, and that in the second group there would be either four or five. The subject was required every time to judge which it was; no answers of "doubtful" were permitted. It was not difficult by means of the switch-board to connect or disconnect a wire while the pendulum passed from the position of contact for one point to that for the other, and any failure to do so was shown by the drum record just mentioned.

Results of the Experiments. The experiments were made upon three subjects, and all numbers from four to ten were used as standards and compared with the numbers greater or less by one. About twenty-four observations were made upon each standard, beginning with four and going up to ten. The order was then reversed, beginning with ten as the standard, and about the same number of observations was made upon all numbers down to four. The following table gives the number of observations and percentages of errors for all the subjects and all the standards.

TABLE I.

Showing number of observations and percentage of errors for all the groups of clicks from three to ten. Interval between the clicks, .011 sec.; between the two groups of clicks, .25 sec. The standard always came first and was compared with either the next higher or the next lower number, but in separate sets of experiments; both higher and lower were not used in the same set.

	H.		S.		B.	
Groups to be Compared.	No. of Observations.	Percentage of Errors.	No. of Observations.	Percentage of Errors.	No. of Observations.	Percentage of Errors.
4—3			48	6.	36	5.
4—5			74	13.	72	12.
5—4	68	10.3	79	24.	48	12.5
5—6	50	26.	58	24.	45	11.
6—5	56	16.	62	16.	46	13.
6—7	54	16.6	53	30.	48	27.
7—6	53	15.1	54	29.	44	18.1
7—8	58	19.	54	28.	48	18.7
8—7	49	6.1	55	13.	48	4.1
8—9	67	20.8	62	33.	48	20.
9—8	52	7.6	46	26.	48	16.
9—10	52	13.4	83	32.	72	18.
10—9	46	19.5	46	33.	54	20.3

From this table it will be seen that on most of the standards, excepting the judgments of S., the percentage of right judgments was greater than 75, the proportion at which knowledge may be assumed to be the basis of the judgment. It is possible then to recognize a difference of one in any number of clicks below ten when the clicks are separated by an interval of .011 sec.

The table also shows that the percentages of errors are greater generally for the greater than for the less number of clicks. This will be more apparent by grouping the percentages on several standards and taking the average of each group.

	H.	S.	B.
First group (standards 4 and 5),	17.2	16.7	13.5
Second group (standards 6, 7 and 8),	13.4	23.2	16.2
Third group (standards 9 and 10)	15.3	31.2	18.5

The strain upon attention was very great, and if any disturbance occurred at the moment the pendulum swung, the judgment was impaired. Very frequently the subject was asked what his opinion of the correctness of his judgments was, or whether any disturbance had distracted his attention. The answers were recorded, and a close correspondence was found to exist between these facts and the accuracy of his judgments. Some observations were taken upon two and three as standards, with an occasional error. With these standards the strain upon the attention was even greater than with larger numbers, and any disturbance was more likely to impair the judgment. In general if judgments could be made immediately they were always more satisfactory; delay was fatal.

It was stated above that the experiments were begun with four as the standard and carried through all the numbers up to ten. The reverse order was then followed to compensate for practice. The results of both are united in the table above. In the second half of the observations the percentages of errors were very much decreased by practice. In the first half H. made 69 errors, S. 113, and B. 61; in the latter half H. made 25 errors, S. 87, and B. 36. On several standards in the latter half, H. and B. made no errors at all. The tendency was to judge more different than alike, though the standard was followed by an equal number, i. e., was repeated, as frequently as by a different number. When the standard was followed by an equal number, H. made 67 errors, S. 96, and B. 63; when the standard was followed by a greater or a less number, H. made 27 errors, S. 87, and B. 39.

In all these experiments the standard had been made to precede the number to be compared. To prevent the possibility of there being a difference in the second group which could not be detected by the record on the drum or the measurements with the tuning-fork, and which served as the basis of judgment, observations were taken in which the number of clicks in the second group remained constant and the number in the

first varied. The standard was eight. The results do not differ from those in the table. S. made 17.5% of errors in 128 judgments, and B. 15.6% in 64 judgments.

Some observations were also taken in which the standard came indiscriminately first or last, with the result that in 64 observations there were 7.8% of errors. The interval between the clicks was .011 sec. The standards were 7 and 8. These experiments are not included in Table I.

All the subjects remarked early in the experiment that when the standard was compared with a less number, they experienced less difficulty in detecting the difference than when the standard was compared with a greater number. The actual numbers of errors from Table I., classified with reference to this point, are found in Table II.

TABLE II.

Giving the number of observations upon the standard and lesser group, and upon the standard and greater group. Conditions as in Table I.

SUBJECT H.

Standard and Lesser Group.				Standard and Greater Group.			
Groups of Clicks.	No. of Obser.	Errors on Standard.	Errors on Lesser Group.	Groups of Clicks.	No. of Obser.	Errors on Standard.	Errors on Greater Group.
5—4	68	5	2	5—6	50	8	5
6—5	56	6	3	6—7	54	8	1
7—6	53	5	3	7—8	58	9	2
8—7	49	1	2	8—9	67	11	3
9—8	52	3	1	9—10	52	5	2
10—9	46	6	3				
Totals,	324	26	14		281	41	13
Percent. of Errors,		8%	4.3%			14.5%	4.6%

TABLE II.—*Continued.*

SUBJECT S.

Standard and Lesser Group.				Standard and Greater Group.			
Groups of Clicks.	No. of Obser.	Errors on Standard.	Errors on Lesser Group.	Groups of Clicks.	No. of Obser.	Errors on Standard.	Errors on Greater Group.
4—3	48	2	1	4—5	74	6	4
5—4	79	11	6	5—6	58	8	6
6—5	62	5	5	6—7	53	6	10
7—6	54	9	7	7—8	54	8	7
8—7	56	4	3	8—9	62	9	12
9—8	46	7	5	9—10	83	13	14
10—9	46	8	7				
Totals,	391	46	34		384	50	53
Percent. of Errors,		11.5%	8.6%			13%	13.1%

SUBJECT B.

Standard and Lesser Group.				Standard and Greater Group.			
Groups of Clicks.	No. of Obser.	Errors on Standard.	Errors on Lesser Group.	Groups of Clicks.	No. of Obser.	Errors on Standard.	Errors on Greater Group.
4—3	36	2	0	4—5	72	3	6
5—4	48	5	1	5—6	45	2	3
6—5	46	4	2	6—7	48	6	7
7—6	44	6	2	7—8	48	4	5
8—7	48	2	0	8—9	48	4	6
9—8	48	8	0	9—10	72	7	6
10—9	54	10	1				
Totals,	324	37	6		333	26	33
Per Cent. of Errors,		11.4%	1.8%			7.8%	9.9%
Grand Totals,		109	54=163			117	99=226

This table means, in the case of H., for example, and the group of clicks marked 5—4, that when he was given by the operator 68 tests, half of which consisted of a group of 5 clicks followed by another group of 5, and half of a group of 5 clicks followed by a group of 4, his description of what had been given him was wrong seven times: five times, when he

was given 5—5, he called it 5—4, and two times, when he was given 5—4, he called 5—5. Similarly with the standard and greater group the first line means that when he was given 50 tests, half being 5 clicks followed by 5 and half 5 followed by 6, he eight times mistook 5—5 for 5—6, and five times mistook 5—6 for 5—5.

The total number of errors upon the standard and lesser group are very much less than upon the standard and greater group (163 to 226), this difference being due almost entirely to the very large number of errors (99) upon the greater group. With two subjects the number of errors upon the greater group is greater than upon the standard, and with all three subjects the number of errors upon the lesser group is decidedly less than upon the standard.

This concentration of the errors was quite unexpected, and seemed so remarkable that the experiment was varied for further investigation of the point.[1] The subject was given a standard followed by the same number, or by a less or greater number, thus making three answers possible. This would make a difference between the greater and lesser groups twice that between either group and the standard. On this account, with standards less than ten, the confusion of the greater with the lesser group, and *vice versa*, was very exceptional.

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[1] In the discussion that followed a brief report of these experiments given before the American Psychological Association at its Philadelphia meeting, Prof. Jastrow objected to the description of this result as "unexpected," since Dr. Hall and he had noticed the same in the course of their experiments on rhythm (above referred to). That they did notice this peculiar constant error I can well believe, but no mention of it sufficient to cause anyone to expect it here is to be found in the very condensed account of their experiments. E. C. S.

TABLE III.

Giving the number of observations and the percentages and numbers of errors, when the standard was compared with the same number or with a greater or a less number. The interval between the clicks was .011 sec., and between the standard and comparison groups .25 sec. The standards were 7, 8 and 9.

	LESSER GROUP.			STANDARD.			GREATER GROUP.		
Subject.	No of Obser.	No. of Errors.	Per Cent. of Errors.	No. of Obser.	No. of Errors.	Per Cent. of Errors.	No. of Obser.	No. of Errors.	Per Cent. of Errors.
H.	36	3	8.3	36	6	16.6	36	8	22.2
S.	21	3	14.2	21	8	38.	20	8	40.0
B.	24	1	4.1	24	6	25.	24	8	33.3
Total.	81	7	8.6	81	20	24.7	80	24	30.0

Total No. of obser., 242; total of errors, 51; percentage of errors, 21.

TABLE IV.

Conditions as in Table III., except that the interval between the clicks was made .0065 sec.

	LESSER GROUP.			STANDARD.			GREATER GROUP.		
Subject.	No. of Obser.	No. of Errors.	Per Cent. of Errors.	No. of Obser.	No. of Errors.	Per Cent. of Errors.	No. of Obser.	No. of Errors.	Per Cent. of Errors.
H.	85	10	11.8	85	26	30.5	85	27	31.8
S.	89	22	24.7	93	36	38.7	89	36	40.4
B.	96	5	5.2	96	31	32.3	96	32	33.3
Total.	270	37	13.7	274	93	33.9	270	95	35.2

Total No. of obser., 814; total of errors, 225; percentage of errors, 27.6.

In both tables the subjects show greater percentages of errors upon the greater group than upon the standard or lesser group, and greater upon the standard than on the lesser

group, thus confirming the results of Table II., and showing that the distribution of the errors does not rest on the particular conditions of the earlier experiments. The percentages of errors for the faster rates are on the whole considerably increased.

An attempt was now made to measure the difference thus discovered by increasing the number of clicks in the greater group. The results appear in Table V.

TABLE V.

Giving the number of observations and percentages of errors, when the greater group was made greater by two clicks than the standard, and by three greater than the lesser group. The interval between the clicks was .0065 sec., and between the two groups .25 sec. The standards used were 6, 7 and 8. With 6 were given 5 and 8; with 7 were given 6 and 9, and with 8 were given 7 and 10 as comparison groups.

	LESSER GROUP.		STANDARD.		GREATER GROUP.	
Subject.	No. of Obser.	Per Cent. of Errors.	No. of Obser.	Per Cent. of Errors.	No. of Obser.	Per Cent. of Errors.
H.	42	14.2	42	11.	42	4.9
S.	30	23.3	31	35.6	30	20.
B.	42	7.1	42	19.	42	2.3

The effect of the double increase of the greater group was to reduce the percentages of errors upon it below either those on the standard or on the lesser group. The percentages of errors on the standard have also been reduced, as was to be expected, for increasing the difference between the standard group and the greater comparison group renders the mistaking of the standard for the greater less likely as well as the mistaking of the greater for the standard.

The proportion of errors made on the standard is worth a little further consideration. Table II. shows that the percentages upon the standard were always greater than upon the lesser group, and with one subject greater upon the standard than upon the greater group. What the explanation of this difference is, is not easy to say, for the subject knew in each case that the two groups would be alike as often as they were different. Possibly some qualitative difference, subjective or objective, between the first and second groups, lies at the bottom of it. It was impossible to do away absolutely with

all such differences, and impossible also to say in what the differences consisted. This may have led the subjects to judge the second set to be different from the first more often than they should. But it may also have been wholly subjective. When the subjects were expecting a difference, they found great difficulty in not imagining one, even though there was none.

A more interesting question at present is, however, the greater number of errors upon the greater group as compared with the lesser group as shown in Tables II., III. and IV. and the effect on the distribution of the errors produced by the increased difference between the standard and greater group shown in Table V. In this case the qualitative difference just considered cannot be made the basis of judgment. It seems rather to be a question of the behavior of the memory after-image. There was a kind of subjective feeling of disappointment when a lesser number followed the standard, and of superfluity when a greater number followed. Possibly the explanation should run something like this: The intervals here dealt with are very much shorter than any we meet with in daily experience, and for that reason in attempting to recall such an interval, we may imagine it too large; we think of it as corresponding to other experiences. This enlarged image of the first interval is brought into comparison with the sensation produced by the second and thus the second seems very much shorter than the first. Our memory image of the first actually increases in length during the interval between the first and second sets of clicks. When the lesser group follows the standard, the second seems decidedly less than the first; but when the same or a greater number follows, the difference is not so great and more difficulty in discriminating is experienced. The experiments of Table V. show that the amount of lengthening in the memory after-image was more than compensated by the increase of the difference between the standard and the greater group. The apparent increase in the first group for standards such as were used must, therefore, be in general less than the time of two clicks, that is, .013 sec. It was supposed that this apparent increase might be greater for longer times than the .25 sec. that in these experiments separated the groups, but when the interval between two groups was increased to six seconds, though a slight increase in the percentage of errors took place, their distribution remained practically the same. The question of fatigue may also enter here as a possible explanation. To produce successively and rapidly a sensation of given intensity, the stimulus must be constantly increased. In order, then, that the second set of clicks shall produce a sensation equal to the

first, the second set must be greater than the first. When the second set of clicks is less than the standard, the sensation would fall far short of the first. This would seem to explain the feeling of relief which followed so perceptibly on the lesser comparison, but it involves the supposition that the number of clicks or the time they occupy is inferred from the intensity of the general effect that is produced.

Tables III. and IV. have been considered so far in relation to the distribution of the errors only. They bear, however, upon the general question of the investigation. Table III. reinforces Table I. in showing that, for two observers at least, a difference of one in excess or defect can be recognized with a standard group of 8 clicks separated by intervals of .011 sec. Table IV. shows that the same is true when the intervals are reduced nearly one-half, namely, to .0065 sec., though the percentage of errors is somewhat greater. Results obtained from experiments where the subject is given three possibilities of answer are not strictly comparable with those in which only two possibilities are offered, but so long as there is no tendency to confuse the greatest and the smallest of the three stimuli (for example, in the experiments giving these tables, to call a 7-click group a 9-click group, or *vice versa*), the case is not very different from that of the two-possibility experiments. In the experiments giving Tables III. and IV. there were very few answers of this doubly wrong kind, and 75% of right answers may be assumed as the measure of discrimination, as in the experiments giving Table I.

With the apparatus used it was impracticable to attempt a further shortening of the intervals beyond .0065 sec.; accordingly attention was turned to the effect of increasing the number of clicks in the groups, with a view to discovering if possible the limit in number for an interval that was easily at command; in this case .0075 sec. = 133 + clicks per sec. Tests were made with groups of 13, 16 and 23 clicks as standards. The results reached are grouped in the following table :

TABLE VI.

Giving the number of observations, the number of errors, and the percentage of errors, when the standard was compared with the same number or with a greater or a less number. The interval between the clicks was .0075, that between the groups .25 sec.

STANDARD 13.

Subject.	Lesser Group.			Standard Group.			Greater Group.		
	No. of Obser.	No. of Errors.	Per cent. of Errors.	No. of Obser.	No. of Errors.	Per cent. of Errors.	No. of Obser.	No. of Errors.	Per cent. of Errors.
H.	17	0	0	24	15	62.5	22	11	50
S.	25	11	44	25	13	52	25	11	44
B.	22	3	13.6	21	10	45.7	21	9	23.3

Total number of observations, 202; total of errors, 83; percentage of errors, 41.9.

STANDARD 16.

Subject.	Lesser Group.			Standard Group.			Greater Group.		
	No. of Obser.	No. of Errors.	Per cent. of Errors.	No. of Obser.	No. of Errors.	Per cent. of Errors.	No. of Obser.	No. of Errors.	Per cent. of Errors.
H.	32	13	40.6	26	13	50	33	15	45.4
S.	35	9	25.7	39	22	56.4	32	12	37.5
B.	29	10	34.8	28	11	39.2	29	12	41.4

Total number of observations, 283; total of errors, 117; percentage of errors, 41.3.

TABLE VI.—*Continued.*

STANDARD 23.

Subject.	Lesser Group.			Standard Group.			Greater Group.		
	No. of Obser.	No of Errors.	Per cent. of Errors.	No. of Obser.	No. of Errors.	Per cent. of Errors.	No. of Obser.	No. of Errors.	Per cent. of Errors.
H.	18	11	61.1	21	12	57.1	22	10	45.4
S.	26	15	57.7	28	17	60.6	26	13	50
B.	17	7	41.1	27	18	66.6	21	8	38

Total number of observations, 206; total of errors, 111; percentage of errors, 53.8.

The percentages that the total number of errors are of the whole number of observations on any standard have increased with the increase of the standard. Errors resulting from a confusion of the lesser group with the greater are much more frequent upon 23 as the standard than upon 13 or 16. The percentages of such errors of the whole number of observations are, on 13, 5.4% ; on 16, 5.3% ; and on 23, 17.4%. The impossibility of judging the difference of one click with a group of 23 when they are separated by .0075 sec., is not to be doubted; only 66.6% of errors would be expected if the answers were dictated wholly by chance. For 13 and 16 the number of right answers is considerably more than could be expected from chance, but considerably less than the required 75%, if we apply the criterion of the two possibility experiments. It would seem then that the ability to recognize a difference of one click under the conditions of these experiments does not extend far beyond groups of 8 or 10 clicks and fails completely at something over 23.

It remains to speak of whether these are time or number judgments. The separate clicks could be apprehended, but counting was out of the question, and it was impossible by any voluntary muscular adjustment to name or record the number. The number of clicks in the group seemed often, if not always, less than the number actually known to be present. The only attempt on my part towards a muscular adjustment was to contract the muscles of the tongue and

larynx twice in rapid succession in such a way as to produce a grating sound when the vocal cords were made to vibrate, thus imitating the groups of clicks to be compared. This took place when I was in doubt. The decision was based upon what seemed to be an equal or an unequal amount of stimulus. An introspective account of how the groups were compared is difficult, because of the extreme concentration of attention required. If the judgment is a number judgment at all, it is something quite different from the number judgments of daily life, possibly akin to the quantitative estimates of groups of objects made by animals and by children before they have learned to count, and, indeed, by adults when for any reason counting is not easy. That time judgments also played a part seems very probable; and the fairest description of the experiments undoubtedly is to say that they are judgments of filled time.

Summary. The experiments that have been described make the following answers to the questions at the beginning of the paper: To question 3 the answer is that the presence of one more or one less click in successive groups can be recognized (probably by the greater length of time required for the larger group) when the number of clicks is not too large and their rate not too rapid. When the rate is 133 per sec., the number cannot very much exceed 10, though with groups of 13 and 16 a difference of one can sometimes be recognized. When the number of clicks is 9 or less the rate may rise at least to 153 per sec., and perhaps higher.

To question 2 the answer is that it is extremely improbable that any clicks are lost in the perception of a rapid group at least up to 153 per sec.; for groups of 8 and 9 can be distinguished at that rate. If the experiments involve a time judgment, it is clear that one one-hundred-and-fifty-third of a second can be recognized, and if so, there would be no need that the images of clicks at that rate should over-lap in consciousness—over-lapping was the cause of loss in Hall and Jastrow's counting experiments. If the judgment was a number judgment, the loss would also seem to be excluded. In some cases where a temporary defect in the instrument caused the actual dropping of a click from the middle of a group, this fact was very easily recognized, though this of course is quite a different thing from the unconscious subjective dropping implied in the question. Particulars as to rates at which this dropping occurred have unfortunately not been preserved, but the statement is believed to be true for all rates.

To question 1 the answer is that for the mere perception of discreteness, number is probably without influence. The fact

that it is less easy to tell 10 clicks from 9 than 5 from 4 shows only that a difference of 1 in 10 is less easy to recognize than a difference of 1 in 5, a result that accords with the psycho-physic law. With reference to the tendency to blend, such a result shows nothing. No greater tendency to blend in the case of the larger numbers was noticed by the subjects. When the question is one of estimating the number of clicks, a slower rate for larger numbers may very well be required.

A few words upon the work of others on related topics will serve to put the results of this study in their proper perspective. Exner found in his studies of reaction-times[1] that his subjects could tell "good" (i. e., quick) reaction-times from slow ones when the difference amounted to about .01 sec., thus seeming to show an ability to perceive a time as small as that amount.

The general form of these experiments is not so very different from those of Dietze's work on the *Umfang* or extent of consciousness,[2] but the intervals used were very much smaller than his, his shortest being .11 sec., or exactly ten times the largest in these experiments. He also found a decided falling off in ability to judge correctly when intervals less than .24 sec. were used, a result different from Mr. Bolton's, but one that may depend either on a difference in experimental conditions, or on a difference in the manner in which groups of clicks of such different rates are judged. Mr. Bolton's subjects found no tendency to such rhythmical sub-grouping of the clicks as Dietze found.

The work of Hall and Jastrow, already mentioned, is somewhat analogous, but different both in the size of the intervals (theirs being .0895 and .0523) and the mental process involved, the effort in their case being generally to count the clicks, or at least to estimate them in numbers. In Mr. Bolton's experiments counting was out of the question. Their observation that filled time seems longer than vacant time could be frequently repeated in these experiments. When, as was natural, attention was concentrated upon the groups of clicks, each group seemed clearly longer than the empty time between them, though with even the most numerous groups the latter was a full third longer. The function of attention in this illusion is certainly a very important one.

Dr. F. Schumann has published two papers on the comparison of small intervals of time; the first in the *Nachr. v. d. Ges. d. Wiss. zu Göttingen*, 1889, No. 20, and the second (a preliminary communication) in the *Zeitsch. für Psychol.* II. 1891, 294–296. Schumann's experiments, however, were made upon empty intervals of time, and though short, did not approach in brevity those of Mr. Bolton, his shortest reported being .15 sec. The same experimenter has repeated in part the work of Dietze with practically the same numerical results, but with a totally different conclusion as to their meaning.[3] Schumann believes that when two such groups are compared the process is somewhat as follows: Each member of the first group as it arrives is registered in memory (in some cases this is clearly done by an accompanying muscular contraction). When the second group is received there is a tendency for the first to be

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[1] Pflüger's Archiv, VI. 1873, p. 613—. But see also an explanation of Wundt's (Physiol. Psychol. 3rd Ed. II. 288), which rests upon other than a time discrimination.

[2] Wundt: Physiol. Psychol. 3rd Ed. II 248–252. Dietze, Philos. Studien II.

[3] Zeitschrift für Psychologie, I. 1890, 75–80.

reproduced in its original number. If the second group contains more clicks than the first, the tendency to reproduction ceases before the cessation of the actual clicks of the second group; if the second contains a less number, the tendency over-runs; if the same number, both tendency and clicks cease together. On these inner experiences the judgment of the subject rests. The "feeling of too much or too little" that Mr. Bolton's subjects speak of, might well point to something of this kind. Its origin, however, could hardly in this case be a muscular response to each click, unless, perhaps, the tympanic muscles respond reflexly, for the most rapid voluntary movements are very much slower than the slowest clicks used in his experiments. If the tendency to reproduce a group of clicks in its exact number is a function of the sensory parts of the cerebrum, there would seem to be no difficulty in explaining the judgment of even these very rapid groups on Schumann's hypothesis.

E. C. S.

STATISTICS OF DREAMS.

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The phenomenon of dreaming has rarely been discussed or investigated in a thorough and in an experimental manner; of description, of theory, of discussion, of poetical analogy and illustration there has been no end; of accurate observation almost nothing.[2] The most scientific books—those of Maury and of Tissié—have been wholly or chiefly the result of the observation of abnormal subjects, and in the interest, more or less distinctly, of pathology. The fullest discussion of the subject—the works of *Radestock* and of *Spitta*—are largely compilations of the recorded dreams of other people.

The basis of the following paper is the accurate record of the dreams of two people, from notes made by themselves during the night, and supplemented by careful study and recollection on the following day. The investigation was undertaken at the suggestion of Dr. Sanford and was carried on for six or eight weeks. Its method was very simple: to record each night, immediately after waking from a dream, every remembered feature of it. For this purpose, paper, pencil, candle and matches were placed close at hand. Early on the next day, with rare exceptions, these abbreviated notes were re-read, copied in full and enlarged by comments, by description of all attendant circumstances, and by the indication in all possible cases of the connection of the dream with the waking life. During the first weeks of the experiments, an alarm clock was used to wake the experimenter at different hours of the night. Later, the use of the clock was discontinued because the excitement of waking through its agency was often so great as to prevent connected memory of the dream.

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[1] Received as a private pupil, 1890-91.

[2] Of this character, I know only the papers of Prof. Nelson (Am. Journ. Psych., I., p. 385); Heerwagen (Wundt's Philosophische Studien, V. p. 88); Ives Delage, Revue Scientifique, July, 1891.

Omitting, so far as possible, all general discussion of the nature and the explanation—physiological and psychological—of dreaming, I shall chiefly discuss the classified results of the observations to which allusion has been made. It will be well, therefore, to give at once the general results of the statistics and to indicate some of the difficulties. Chief among these is that of recalling dreams after one has waked. Doubtless this is due, both to the lack of congruity between the waking and the dreaming life, and to the slight excitement which often accompanies the act of waking. To recall a dream requires usually extreme and immediate attention to the content of the dream. Sometimes the slight movement of reaching for paper and pencil or of lighting one's candle seems to dissipate the dream-memory, and one is left with the tantalizing consciousness of having lived through an interesting dream-experience of which one has not the faintest memory. To delay until morning the record of a dream, so vivid that one feels sure of remembering it, is usually a fatal error. During the progress of the observations, the account of one dream, apparently of peculiar significance, was written out in the dark by the experimenter, who then sank off to sleep with the peaceful consciousness of a scientific duty well done. In the morning the discovery was made that an unsharpened pencil had been used, and the experimenter was left with a blank sheet of paper and no remotest memory of the dream, so carefully recalled after dreaming it.

The difficulty in remembering dreams suggests, of course, the impossibility of an exhaustive enumeration of their peculiarities and of any positive conclusions from the figures of such tabular views as will be offered. At the best, one may discuss only dreams as remembered, and the power of recollection varies widely with age, temperament, health and other conditions. A dream which is remembered in detail must have been a vivid one, but it is not true, as we often assume, that a vivid dream is necessarily well remembered, for both dream-records here considered contain several cases of "vivid dreams immediately forgotten." In one case a distinct memory of a vivid visual dream of a newspaper included no faintest recollection of one word on the page. It seems certain, therefore, that most people dream much oftener than they think.

While some dreams thus escape the memory, the very effort to record may well tend in the contrary direction, toward an increase of their number. The observer with the recording "on his mind" may sleep less soundly, dream more and wake oftener. Too great an anxiety certainly has this effect. Finally, the student of dreams is

in danger of reading into his dream much that is characteristic of the waking consciousness, something as one interprets an animal's actions by one's own. As the dog is said to "reason" when he performs—instinctively, for aught we know—an act which would imply reasoning on our part, so emotions, thought and deliberation, which were really absent from a train of dream-images, may be supplied by the reflective after-consciousness.

With these preliminary cautions one may turn to the study of the figures. The observers will be designated as S. and C. S., a man of thirty-two, took notes for 46 nights and recorded 170 dreams; C., the writer, a woman of twenty-eight, took notes on 55 nights and recorded 205 dreams, an average of nearly four dreams a night in each case. Neither observer had previously regarded himself as a frequent dreamer. After the records were completed, each worked up the figures for his own dreams, with occasional conferences with the other. Such individual treatment was unavoidable where the relations of the content of the dreams to previous experience were to be considered; some differences were thus introduced into parallel tables, but none of them are regarded as of great importance.

TIME OF DREAMS.

TABLE I.

Observer S. Number of Dreams Included in the Table, 118.[1]

10.30 P.M.—11.30 P.M.	11.31 P.M.—5 30 A.M.	5 31 A.M.—8.30 A.M.
4 3.4%	52 44%	62 52.6%

TABLE II.

Observer C. Number of Dreams Included, 183.[2]

BEFORE 4 A.M.			AFTER 4 A.M.	
On Fal'g Asleep Eve'g.	11 P.M.-2.30 A.M.	2.31 A.M.—4 A.M.	4 A.M.—6 A.M.	6.01 A.M.—8 A.M.
1	22	26	78	56
49 (26.8%)			134 (73.2%)	

[1] The records on the remaining 52 dreams did not specify the time with sufficient exactness for use in this table.

[2] The total number of dreams recorded was 205. The last 7 dreams recorded together with 15 others have been omitted from this count, because no sufficient record was made of the time at which they occurred.

The general result of these tables is, of course, to confirm the opinion that most of our dreams occur during light, morning sleep. They show, however, conclusively that the sleep of the middle of the night is in no sense a dreamless sleep, for, of course, the recorded number of "before 4 A. M." and "before 5.30 A. M." dreams is only a record of remembered dreams, and after the use of the alarm clock had been discontinued, fewer dreams from the middle of the night were recorded because of the infrequency in waking.

The inquiry whose results are next tabulated was undertaken in order to discover the effect, if any, of the time of a dream, on its connection with the previous waking-life.

TIME VERSUS SUGGESTED CHARACTER OF DREAMS.

TABLE III.

Observer C. 194 Dreams Included.[1]

	BEFORE 4 A.M.			AFTER 4 A.M.		TIME UNDET'D.	TOTALS.
	Falling Asleep	11–2.30	2.31–4	4.01–6	6.01–8		
No Suggestion Traced.		1	4	11	5	1	22 (11.3%)
Slight or Vague Suggestion or Mere Congruity.		8	11	33	22	6	80 (41.3%)
Suggestion.	1	7	8	26	17	5	64 (33.%)
Close Suggestion.		6	2	7	10	3	28 (14.4%)
Totals.	1	22	25	77	54	15	194
	= 48			= 131			

[1] The total (194) differs from that of Table II., because 4 dreams, occasioned by peripheral sensation, have been rejected from the count, and 15 of undetermined time included.

The most obvious conclusion from this table is the close connection between the dream-life and the waking-life. In only 11 per cent. of the dreams is it impossible to discover such connection. When, however, we seek the answer to our immediate question, it seems impossible to calculate accurately the influence of the time of a dream upon the degree to which it is associated with the waking experience. It is impossible to conclude that the dreams which follow most immediately upon our daytime experience will be more closely united with it than the morning dreams ; and equally wrong to conclude that dreams during the lighter sleep of morning, nearer the waking-state, will be more congruous with the waking-life.

TABLE IV.

Observer S. 170 Dreams Included.

TIME.	10.30—11.30	11.31—5.30	5.31—8.30	UNDETERMINED.	TOTAL.
No Suggestion Traced.	1	29	34	25	89 (52%)
Vague Suggestion or Mere Congruity.	1	13	21	16	51 (30%)
Suggestion Traced.	2	10	7	11	30 (18%)
Total.	4	52	62	52	170

The figures in Table IV. are of the same general import as those of Table III., though the proportion of dreams suggested by waking-life is much smaller. This is undoubtedly due in part to a different criterion in classifying. The dreams of S. were very frequently pervaded with an atmosphere of experiment, not definite enough to warrant placing them in the class of vague suggestion, but undoubtedly dictated by the waking occupation, though possibly also by the very attempt to record the dreams. Furthermore, dreams in which the companions of waking-life were present were not included as suggested dreams unless some incident of the dream was also suggested.

The next tables embody an attempt to classify the dreams according to their vividness. The difficulty of this classification has been already suggested. Vividness and the fact of

being distinctly recalled are not equivalent characteristics. Because a dream is indistinctly remembered, it is not fair to conclude that it is not a vivid one, yet it is not safe to trust altogether to a mere memory of vividness without any support for the impression. The degree to which a dream is recalled (which may be roughly gauged by the length of the written record) is thus a convenient, though not an absolute, test of the vividness of a dream.[1] Both factors enter into the following classification: Class I. includes dreams which were very vivid and were remembered in detail; class II. comprises vivid dreams, usually less vivid than those of the first class, and less accurately remembered; class III. includes, in general, dreams of still less vividness which are recalled in less detail, but contains a few cases of dreams which are largely forgotten, though remembered as vivid;[2] finally, class IV. contains the record of dreams largely forgotten, which—so far as recalled—were indistinct.

VIVIDNESS OF DREAMS.

TABLE V.[3]

Observer S. Number of Dreams Included, 170.

	CLASS I.	CLASS II.	CLASS III.	CLASS IV.	TOTAL.
No. Cases.	40 (23.5%)	41 (24.1%)	44 (25.9%)	45 (26.5%)	170
Total No. of Lines.*	1062	331	128	96	1617
Average No. of Lines.	26.5	8.1	2.9	2.1	9.5+

* Average number of words in a line of record, 12.2.

[1] The plan of estimate by the length of the record is borrowed from Prof. Nelson. Am. J. Psych. I. p. 383.

[2] For instance, the newspaper dream mentioned above.

[3] So nearly as I can judge, vividness, as opposed to length of record, was a little more closely the standard of classification in Table VI. Notice the greater length of dreams in Table V., except under class III.

TABLE VI.

Observer C. Number of Dreams Included, 191.[1]

	CLASS I.	CLASS II.	CLASS III.	CLASS IV.	TOTAL.
No. Cases.	32 (16.8%)	65 (34%)	57 (29.8%)	37 (19.4%)	191
Total No. Lines of Record.*	705	443	174	66	1388
Average No. of Lines.	22	6.8 (circa)	3	1.75 (circa)	7.25 (circa)

* Average number of words in a line, 8.6 (circa).

In general, the dreams of C. were written out in rather less detail than those of S., so that the comparative length of the records does not give an accurate idea of the comparative length of the dreams of the two observers. A notion of the principle of classification may be gained by referring to dreams 43, 193 (C.); 28, 111 (S.),[2] which have been included in class I.; to dreams 91 and 94 (C.),[3] which belong to class II., and to dreams 19 and 68 (C.),[4] of class III.

These results are perhaps fairly indicative of the average vividness of dreams. The following table, and the inference which may be drawn from it, suggests that vivid dreams are by no means confined to morning hours, as might perhaps be supposed :

[1] Fourteen dreams, in which the record is inadequate, are omitted.
[2] Pages 336, 328, 325, 324. Observe that only dream 43 is quoted in full.
[3] Pages 326 and 325. See also dreams of S. in foot note, p. 132.
[4] Pages 343 and 336.

DREAMS BEFORE AND AFTER 4 A. M.

TABLE VII.

Observer C. Number of Dreams Included, 184.[1]

	CLASS I.	CLASS II.	CLASS III.	CLASS IV.	TOTAL.
Before 4 A. M.	2 (4.26%)	10(21.28%)	20(42.55%)	15 (31.91%)	47
After 4 A. M.	27 (19.71%)	54(39.41%)	33(24.09%)	23 (16.79%)	137
	29 (15.76%)	64(34.78%)	53(28.81%)	38 (20.65%s	184

DREAMS BEFORE AND AFTER 4 A. M.

TABLE VIII.

Observer S. Number of Dreams Included, 109.[2]

	CLASS I.	CLASS II.	CLASS III.	CLASS IV.	TOTAL.
Before 4 A. M.	5 (15.6%)	9 (28.1%)	8 (25%)	10 (31.3%)	32
After 4 A. M.	24 (31%)	25 (32.5%)	15 (19.5%)	23 (16.9%)	77
	29 (26.6%)	34 (31.2%)	23 (21.1%)	38 (21.1%)	109

It will be observed that as large a proportion of dreams of a moderate degree of vividness (Classes II. and III.) occur during the night as toward morning. The percentage of most vivid dreams is greater after 4 A. M.; that of the least vivid dreams, on the contrary, is greater among the night dreams

[1] The dreams omitted from the count in Table VII. are the first fourteen, of which the record is not an adequate one; and the last seven, in which the time recorded was not kept.

[2] The dreams omitted are 43, whose time is not recorded; and 17, whose time (as between 11.30 and 5.30) cannot be classified on this plan.

than among those of the morning. We conclude, therefore, that the dreams after four o'clock tend to be more vivid than the earlier ones, but that the distinction is no absolute one.

Dreams may be most conveniently classified according as they are connected through sense excitation with the immediate present, or through the fact of association with the waking life of the past, according, that is, as they contain presentations, or merely representations. Or, in physiological terms, dreams are occasioned or accompanied by peripheral or by merely cerebral excitation. When I dreamed of hearing fire-bells, and then seeing from my window the burning of a church, and waked to find my window-shade flapping back and forth in a strong wind, this was evidently a presentation dream; on the other hand, when I dreamed of meeting Caroline Schlegel, after actually reading a story of her life, it was a representation dream. But as Spitta (who makes a similar division into *Nervenreizträume* and *Psychische Träume*) suggests[1] no dream belongs exclusively to one class or to the other, so that it is more accurate to refer to the presentation elements and to the representation elements in a dream.

The following are the classified results of the dream-records under this head:

PRESENTATION ELEMENTS IN DREAMS.

TABLE IX.

Number of Dreams Included: For S., 170; for C., 165.[2]

	VIS'L.		AUD.		TACT.		TEMP.		GUST.		ORGAN.		TOTAL.	
	Clear	?	Clear	?	Clear	?	Clear	?	Clear	?	Clear	?	Clear	?
S.	1	1	3	9	1	2		4				1	5	17
C.	1	1	2		1	1	1	1	2		1		8	3

Totals, S., 22=13.2%.
C., 11= 6.7%.

The most striking outcome of the figures is the relatively small number of dreams which can be shown to include any sense-perception. A closer inspection shows that, with both observers, the auditory dreams are of chief importance, a fact

[1] Spitta, p. 213.
[2] Dreams of Class IV. were not considered in C.'s Statistics.

of common experience and readily explained since, next to sight, hearing is the sense most constantly exercised, and of the two is the more easily stimulated when the eyes are closed in sleep. Visual presentation dreams are less frequent, but dreams from dermal excitation occur comparatively often, and the occasioning sensation is apt to be much exaggerated. For instance, a dream of struggling to crawl from an elevator, through an absurdly small opening, into an eighth-floor apartment, was traced directly to a cramped position and to a heavy covering across the face; a dream of a sleigh-ride on an intensely cold day, and of hearing that the cornea of a friend's eyes had broken from the cold, was evidently occasioned by a stiff breeze blowing in at the window. Nightmares are often attributed to this cause, as the German name *Alpendruck* shows.

Dreams through stimulus of the lesser senses, smell and taste, are infrequent, but not unknown. Our observed dreams include none through stimulus of the sense of smell, but there are two clear instances of the taste-dream, though in most so-called taste or smell-dreams one dreams of seeing food or flowers, not of actually tasting or smelling.

Of dreams whose starting point is organic sensation[1], there are only two cases among those classified in Table IX. Common instances of such dreams are fright dreams in their simple form, or in the exaggerated nightmare stage,[2] which are largely due to organic sensations of pressure for breath, of quick heart-beating, or of digestive discomfort. Our records contain no instances of dreams initiated by actual movement.

So nearly as I can judge, the intrusion of the sense-impression into the dream-train is during the transition between sleep and waking, and the dreams or parts of dreams preceding the waking dreams are those undisturbed by sense. In any case, the emphasis, by most writers,[3] of dreams of this nature seems to be due to the fact that the dreams from which conclusions have been drawn are, in almost all cases, particularly striking and unusual dreams, and therefore not fairly representative of the warp and woof of dream experience. The most important feature of the present investigation is really the very prosaic and ordinary nature of most of the dreams recorded and the exhaustiveness with which, during a number of weeks, all remembered dreams, and not merely peculiar and abnormal ones, were registered.

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[1] For a fanciful account cf. Scherner.

[2] Cf. Radestock, *op. cit.*; and Kant, Anthropology, § 34, p. 105.

[3] *E. g.*, Sully, Illusions.

The representation elements of a dream may enter through association of the first stage of the dream with the waking life, in which case we have what is properly a cerebral or representation dream, or may enter through association within the dream of one element with another. In this latter sense, all dreams contain representation elements, for the very transformation of the sense-excitation of the presentation dream into an image requires the associative process.

REPRESENTATION DREAMS.
TABLE X.

	Representation Dreams.	Presentation Dreams.	Undetermined.	Totals.
S.	148[1]	22		170
C.	191	11	3	205

The following table presents a classification of what we have called representation elements of dreams:—

REPRESENTATION ELEMENTS[2] IN DREAMS.
TABLE XI.

	REPRESENTATION OF COGNITION.						REPRESENTATION OF ACTION.	
	Vis'l.	Audit.	Derm.	Gust.	Olf.	Gen'l.	Vocal.	Other.
S.	113	76	7	0	2	1	36	44
C.	127	81	14	0	2		71	71

Total number of dreams considered: S., 133; C., 165.[3]

The preponderance of visual images is very striking. The relation to auditory images (the figures indicate in both instances a ratio of about three to two) seems to me the more

[1] All which cannot be counted as presentation dreams have been counted as representation dreams.

[2] The table is not a classification of dreams, but of representation elements. Representations of more than one sense and of action often occur in the same dream. Hence there can be no attempt to make totals correspond.

[3] All dreams of class IV. have been omitted from the total 205 dreams because too imperfectly remembered, with a few others not classed as IV., but incompletely described (in all, 40).

remarkable in the case of C., who has no very vivid, visual imagination, but on the other hand thinks habitually with words rather than with concrete images.

Next to visual-dreams, word-dreams have been by far the most frequent, in the case of C., and have occurred often with S. There are many cases, in the former records, in which the words have seemed to suggest the dream or to form its significant part. The words have been spoken, heard, read and sometimes written; very often they have been spoken and heard, or read and written, or spoken and read, in the same dream. The following table summarizes these results:

WORD ELEMENTS[1] OF DREAMS.

TABLE XII.

	Spoken.	Heard.	Visibly Read.	Written.	
				Clear	?
S.	36	63	10	4	2
C.	73	78	11	2	

Total number of dreams in which words occur: S., 77 (of 147);[2] C., 110 (of 165).[2]

English was the language of all these dreams, but in the following cases foreign languages were represented in the dreams of C., usually only by a word or a phrase, but sometimes more at length: German, 5 cases; French, 5 cases; Italian, 1; Latin, 1.

C. had many dreams in which the connection with waking life and the occasion of the whole dream or of some part of it was a merely verbal association. Some of these dreams consisted wholly of the philosophical expressions, more or less logically combined, of the authors whom C. was then very intently studying. In one case the application of such a philosophical phrase was very oddly turned. C. had been reading Fichte's *Wissenschaftslehre*, and the day before had wrestled with his description of the non ego as "*bestimmbar aber nicht bestimmt.*" In the dream, C. was discussing with a friend a matter of personal expenditure and told him, quite properly, that it was "*bestimmbar aber nicht*

[1] Many dreams contain both spoken and heard words, etc.

[2] Dreams of class IV. not included. 14 dreams included, which were omitted from Tables VI. and VII. because time-record is not given.

bestimmt." Another time, after reading in the evening that Elizabeth Phelps Ward had bought a farm at Oak Hill, a dream followed of lunching at the Oak Farm restaurant.

Sometimes a word was visualized in a dream. C., for instance, after reading just before falling asleep a list of words of which one was "snake," dreamed of walking through tall grass and of seeing a snake in the path, and after reading Everett's Fichte, dreamed of meeting Dean Everett. Occasionally the dream began as a verbal one and grew from word to act, as when from the reader of a romance, the dreamer became one of its actors. The following table covers most of these cases.

TABLE XIII.

Observer, C. 28 cases included.

- Words suggest dreams in 28 cases.
  - Words are transferred in 17 cases.
    - With same application, 14 cases.
      - Philosophical application: clear, 8; doubtful, 4.
      - Other application, 2.
    - With changed application, 3.
      - Philosophical application, 1.
      - Other, 2.
  - Words are visualized in 3 cases.
  - Words enter otherwise, 8 cases.

Words are exactly remembered in nineteen cases by S., in five only, by C. Three of these are cases of puns, which would never have been perpetrated in the waking life.

II.

An important result of the study of dream phenomena is the demonstration of the essential congruity and continuity of the dream life with the waking life. The stages and aspects of psychic activity which make up our dreams are recognized as those of which our whole conscious life consists, and a careful examination of dream phenomena tends to establish what Spitta calls the "complete unity of the human mind."[1]

Of course perception occurs only in "presentation dreams," in which actual sensation has stimulated the dream activity, and the characteristic sort of dream-consciousness is the imaginative. A dream is in fact most simply defined as an illusion of the imagination in which images are reproduced, accurately and completely, or in fresh and varying combination. C. twice dreamed in exact detail some immediately preceding

[1] "Völlige Einheit der menschlichen Seele."

event, a case of the simplest sort of mechanical imagination,[1] in which the association was that of total recall.[2] Far more often the representation of parts of one experience is combined with the representation of congruous parts of another, and most often the imagination reaches the emotional and erratic plane of fancy.

But besides this unrecognized imagination, which is the dream activity as such, there is overt imagination, just as there is memory, within a dream. In illustration of this, compare the following dream record—an extract from a longer one.[3]

April 7–8, 6.50 A. M. Observer S., Dream 111. "Before that, had been camping out with a lot of '83 people and others that I did not know, not far from where the gypsies used to camp on Lake M. - - - - -. I think I said I would stay as long as anybody at the camp, and wondered how it would seem at last when everybody had gone, and if it would not then seem good to get back to a boarding-house. (I thought of the one to which I should go in O., and also thought how things would have to be raked up about the camp, in the dust)". The parenthesis contains the record of a distinct case of imagination within a dream.

Accurate memory of real events in the waking life is not frequent, but occurs in well-marked cases. For instance, C. twice recalled incidents appropriate to the dream situation, of travel in Italy four years before; in another case, two factory-girls applied for help in getting work, and C. remembered the circumstances of certain actual labor troubles in Lynn, which made it unwise to send them there.

The recorded dreams abound in instances of false memory, paramnesia. Dream persons and dream places are remembered, on waking, to have been widely different from the real people and places. Or, the dream consciousness accounts for the presence of some unexpected person, for some unfamiliar detail of the surroundings, by "remembering" something which never really happened.

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[1] It is misleading to call this, as Maury does, "souvenir ignoré," or "mémoire . . . non consciente." Memory is distinguished from imagination in that it involves conscious reference to the past and to the self.

[2] Cf. James, Psych., I., p. 588.

[3] All the dreams quoted from the records have been copied exactly, except for amendments of the English, not affecting the sense. Dashes (- - -) indicate omissions, by the editor, of matter irrelevant to the case in hand; asterisks (* * *) are copies of those *Gedankenstriche*, made in the original records.

Not only imagination, but real thought occurs in dreams, though the fact is often denied.[1] The frequency of conversation in dreams might be looked upon as a proof of the existence of the lower stages, conception and judgment, of thought-activity, for both are necessary to the use and combination of words. But such dreams may be instances of merely visual or auditory memory of words, and if there were no instances of explicit judgment and of reasoning in dreams, the presence of thought could hardly be proved. But not only do we form judgments in our dreams, sometimes correctly, though often inaccurately, but we carry on whole trains of reasoning, reasoning sometimes incorrectly, often correctly from absurd premises; and less often both correctly and from natural premises. (Cf. the dream quoted on page 328.) From many illustrations in the dream-records, I select the following:—

Incorrect Reasoning. Tuesday, March 31, 6.50 A. M. Dream 94. Observer C. "When we think substantiality and causality we think that which is out of time—but time we know exists, therefore substantiality and causality must be given up. This is Mr. Huxley's argument."[2]

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[1] Cf. Spitta's dogmatic statement, p. 127 (see also p. 139); "Die traümende Seele ist unfähig richtig zu schliessen."

[2] An instance of correct and ingenious reasoning in the dream of S. I quote because the circumstances are so extraordinarily improbable, though its very elaborateness lays it under a certain suspicion of unconscious editing. March 15-16. Time, undetermined. Dream 28. (Observer S.) "In the sitting-room of the 13th street house. L. [a cousin] is there with her baby, and about to fit him with a new skull. The skull is a great deal too large for such a little baby (it was one which had belonged to some relative of the child, that had died sometime before; the skull was white and nicely cleaned like an anatomical specimen). I reflected that this was probably frequently done, and for this reason one sees so few skulls of children—they are transplanted to the heads of other children. I remembered that Dr. Z. had only one - -. I disjointed the skull at the sutures, and it came apart nicely. L. took the parts and went with the baby and some one else out of the room to perform the operation. I wondered at their coolness in prospect of it. Presently they returned with the baby. The child seemed a little upset, but not in a collapse, and only showed a partially healed wound over the top of the head from side to side where the scalp had been cut. They were not even going to keep the child indoors, but gave him to me to carry out. I set off with him toward the high school, and after a little found that he could talk, though making some errors, as if not entirely used to talking. As we continued he talked more and grew considerably (though I didn't notice it at the time, and only now remember that he was a good deal bigger on the return than on setting out). The theory seemed to be that by getting an older skull he came into, or rapidly adapted himself, to the size and attainments of the previous owner of the skull. I was struck with the fact that this put the active and organizing principle—the soul—in the skull instead of the brain—and naturally was filled with wonder."

The following table includes these cases of imagination, of memory and of thought in dreams. So constant an effort has been made not to supplement the dream facts, by waking experiences, that the figures are rather too small than too large:—

MEMORY, IMAGINATION AND THOUGHT IN DREAMS.
TABLE XIV.

Number of Dreams Included: S., 133; C., 165.

	Memory.			Explicit Imagination.		Thought.	
	Accurate.	Paramnesia.					
		Clear.	?	Clear.	?	Clear.	?
S.	12	27	6	10	1	23	12
C.	12	23	2	7	4	19	10

Dream 94, quoted on page 325, from C.'s dreams, is one of twenty-four called "philosophical." The record of the shorter dreams among them suggests at once that they were really not in the least philosophical or reflective in character, but mere parrot like memory of the words met over and over again in reading. I am inclined to the view that this is often the case, but a dream like "Mr. Huxley's argument," quoted above, is evidently one of thought, however inaccurate, on a metaphysical subject. Another case of the same sort is Dream 91, March 31, 6.50 A. M. (Observer C.). "Father is arguing with certain people, especially with Mr. S. One point emphasized is causality. I like father's general line of thought, but object to his view on this point. I say, 'May I speak on this subject?' Father is doubtful if there is time. I begin, 'What means a law of thought?'

Mr. S.—It means nothing.

I—But it stands for a fact. What fact? Causality is a way in which the mind acts * * * * *.

A lady present compliments father on his presentation [of the subject]."

The so-called philosophical dreams of C. have been classified as follows :—

PHILOSOPHICAL DREAMS.

TABLE XV.

Observer, C.

The Philosoph. Character May be Simply a Verbal Memory. 16.		The Dream Involves Thought. 8.	
Clear.	?	Clear.	?
11	5	6	2

The emotional quality of dreams varies greatly with different persons and with different conditions. C.'s dreams, though they are seldom actually painful, are apt to be pervaded by a generally unpleasant feeling, often rising of course to the stage of positive and excessive discomfort. To certain friends, however, dreaming is an almost uniformly pleasant experience.

The following table gives the meagre results of the attempt to define the prevailing emotions of the recorded dreams :—

EMOTIONS IN DREAMS.

TABLE XVI.

		S.	C.
Pleasure,	Clear,	2[1]	3
	Doubtful,	—	4
Neutral—Surprise and Excitement,		3	2
Pain,	Fear and Repulsion,	2	6
	Shame and Mortification,	3[2]	11
	Disappointment and Regret,	—	9
	Discomfort,	2	5
	Anger,	4	5
	Remorse,	1	—
	Jealousy,	1	—
	Perplexity and Hurry,	—	9
	Uncertain,	3	6
Totals,		20	60

Indications of volition in dreams are rarer, yet each dream record includes a few cases of well marked deliberation followed by decision, so that one may safely deny the assertion

[1] Includes one of class IV.
[2] Includes one counted before, so not added to total.

often made that the will is never active in dreams. In the following dream there was distinct consciousness of an effort of will :—

Thursday, May 7, 5.30 A. M., Dream 193. (Observer C.) "I am teaching a Greek class, a freshman college class, yet it appears to be beginning Greek. It suddenly occurs to me that they can better and more easily learn the Ionic dialect in connection with the Attic. So I tell them to open their Hadley and Allen grammars at the proper point, and say: 'Now, if you are not in my class next year, how surprised your teacher will be at what you know about Herodotus.' . . . . At this point, a strange woman enters with some one (whom I do not know, but who appears to have some connection with the college). She begins to address the class apparently for some philanthropic object. I step forward and say, 'I am the instructor of this class. Will you show your authorization for interrupting it?' She answers that she has had permission, and finally shows me a certificate dated 1882, and signed, A. E. Freeman. I tell her that the date is too early to be satisfactory. She says: 'But this permission was a renewed one,' and shows me an earlier one. I tell her that the permission is insufficient; but she begins to speak. I interrupt her and say, 'Now one of three things will occur: I shall interrupt you, or I shall get some one to stop you, or you will stop of your own accord.' The woman argues with me; she is poor and needs money. [Note, added later: I am greatly distressed and divided between my wish to be generous and my conviction of the impropriety of her proceeding]. I say, 'No matter; you should have the president's permission' - - - She finally goes."

The study of association in dreams is particularly interesting, though very elusive. One may notice not only the association of the dream with the waking life, but that of the elements of a dream with each other. Its chief characteristic is the spontaneity resulting from the lack of fixed attention in dreaming, and from the absence of standards and norms of objective truth. This spontaneity, however, does not necessarily interfere with the consistency of plot, so to speak, in a large number of our dreams, which preserve one or all of the unities of time, place or interest, so that they may be called by Sully's term, "dramatic dreams."[1]

The spontaneity and freedom of dream-association are due, as we have said, to the general lack of sense-impressions. Sometimes, however, the associative train is sharply turned

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[1] Illusions, pp. 172-177, especially p. 175.

by the intrusion of presentations,[1] not recognized as such, or by irrelevant associations, to which insufficient selective judgment is opposed: there result what Spitta calls the "*Ideensprunge*" of dreams. The frequent breaks in the continuity of dream-life make it impossible to classify dream associations after any of the ordinary methods. Often, however, they are emotional. The persistence of some feeling of the waking life seems to determine the character of the associated dream features. From this source arise the dreams of presentiment, according either with hope or with fear.

Two quantitative distinctions of association are constantly illustrated in dreams. We often[2] re-live in dreams, in exact detail, scenes through which we have just passed. More often, as we have said, one very small element of a dream-image is accentuated, and followed by an associated image of which the greater part in turn fades away, leaving a small portion which is followed by its congruent image. In the first case, we have instances of total redintegration. The second illustrate modulation in association,[3] continued focalized redintegration.[4] It is needless to give examples, for everybody recognizes this quality of his own dreams. All the absurdity or incoherence of dreams is accounted for by this feature of the associative process.

The definite question of the subject matter of our dreams is partly answered through our dream-statistics about the persons, places, time and character of these dreams. The lists of *dramatis personæ* are very long, and suggest that the dream world is well peopled.

[1] Spitta calls attention to the analogous waking experience "Das Selbsthewisstsein findet sich selbst bei allseitigen Wachsein in einem ununterbrochenen Kampf mit der aüsseren und inneren Sinnenwelt."

[2] C.'s dreams include two such cases.

[3] Dr. Scripture's term.

[4] James, Psych., I., p. 578.

TABLE XVII.

Observer S.

Number of times of dreaming about people.

Family, (3)— (A 14, B 8, C 1)—23.		
Professional associates, (20)—56.		
Friends,[1] (69)	In W.,	44
	B.,	8
	H.,	4
	C.,	20
	Others,	18
		94
Unknown,		55
	Total,	228 instances.

TABLE XVIII.

Observer C.

Number of times of dreaming about people.

Family, (5)—A 25, B 7, C 11, D 7, E 3—53.			
Professional associates, (4)—			18.
Friends,[2] (140)	S.,	31	
	In N.,	137	
	" W.,	22	
	" C.,	3	
	" B.,	12	
	Others,	21	
			226
Unknown,			63
	Total,		360 instances.

In the following tables the letters have the same signification as in those last presented :—

[1] Of the letters under this head: W. refers to the present residence; B. to immediately preceding place of residence; H. to a place where two years were spent after leaving college; C. to the place where the childhood and greater part of the observer's life have been passed. The numbers in parentheses refer to the number of different people.

[2] The numbers after the headings refer (as above) to the number of different people. Of the letters under the head "friends," S. refers to one very intimate friend, very often seen, who figured more frequently than any other one person; N. refers to the present residence; W. to the institution in which the observer has been instructor; C. to college town; B. to the home in childhood.

LOCALE OF DREAMS.

TABLE XIX.

Observer S.		Observer C.	
W. and vicinity,	44	N. and vicinity,	78
B. " "	4	W. " "	7
H. " "	3	B. " "	9
C. " "	21	Other places,	11
Other places,	4		
			105
	76	Place unknown,	39
Place unknown,	57	No place,	21
	133 instances.[1]		165[1]

By far the most conspicuous feature of this place classification is the prominence of places associated with the life of the present. This tallies exactly with the result of the dream census of persons, by which the associates of our dreams are seen to be in large number the companions of the every day life of the immediate present; and the result falls in also with the surprising conclusion from the attempt to classify dreams according to the time in which, as one dreams, one supposes oneself to be living. In all the 375 cases which our records include, there is no instance of a dream in any other than the present time. When the dream was of the childhood's home, or of some person who had not been seen for many years, the apparent age of the dreamer was never lessened to avoid anachronism. Whatever the place or character, the subject was always at his proper age and in his general condition of life (excepting, of course, in the few cases of changed identity).

It is thus evident that the dream is connected with the waking life, and—in the experience of these observers—usually with the recent life. But what besides recency determines the exact line of association, the particular elements which, in a sense,[2] are reproduced in the dream? If we were discussing waking association, we should name two other factors, vividness and frequency, and should especially emphasize the fact that the subjects of conscious thought or imagination are those in which we are most deeply interested. The first impression is that the same holds true of dreams, and that the subjects of dreams are those in which our waking interest is centered. The opinion seems to be strengthened by the fact

[1] Dreams of class IV. are omitted, though they include some cases. Of C.'s dreams, two are omitted from class N., because they belong also to class "B" and to class "Others."

[2] Not a strictly philosophical sense.

of the prominence in dreams of well-known people and places yet a careful examination of one's dreams contradicts this view, as is suggested in the following table:—

CONNECTION OF THE DREAM WITH LIFE.
TABLE XX.

	Personal.		Professional.		Trivial.	
	Gen. char.	Elements.[1]	Gen. Char.	Elements.	Gen Char.	Elements.
C.	57		44	82	104	161
S.	29		50		86	

This table is a very inadequate one, because of the difficulty of defining closely the terms, especially "personal" and "trivial." The former is used, in general, of dreams which are individual in their character, essentially connected with oneself, so that they could hardly be dreamed with reference to another person. Such are mostly dreams about one's own home. The trivial or accidental dream or dream-element is one so unimportant that it might equally well be dreamed with reference to another person or situation.[2]

The figures, however little one emphasizes their rigid accuracy, certainly indicate, what any one may verify by examination of his own dreams, the constancy of the trivial and the accidental, the tendency of the unimportant events of the waking life to reappear in our dreams, and the surprising fewness of the cases in which the dream is associated with that which is of paramount significance in one's waking experience. The so-called "personal" dreams seldom, if ever, refer to really important phases of personal relation.

A little reflection on the subjects about which one does not dream cannot fail to strengthen this view. In times of be-

[1] "Elements" means "parts of a dream." The classification is used with reference to dreams in which the predominant character differs from some of the parts.

[2] The following are examples of dreams classified under these heads:

Personal. (C.) March 28. "Mrs. S. lunches with us and we have coffee cups which do not match."

Professional. (S.) March 30-31. "Dreams of demonstrating the Wheatstone stereoscope to Prof. and Mrs. X. Neither person well marked in my dream."

Trivial. (S.) March 29-30. "Dreamed of a large party (large as a S. S. picnic), being out boating on L. pond, or some place like it. The D.'s and others seemed to be there.

reavement one seldom dreams of the dead. The fact is often denied, chiefly because of the overwhelming impression produced by the infrequent dreams of this nature, yet it is proved by the experience of all whom I have questioned, by the examination of the dream records, and by the careful inquiries of M. Ives Délage,[1] the only writer, so far as I know, who has faced this problem. Monsieur Délage was profoundly impressed by the fact that during a long time of deep, personal sorrow, he never once dreamed of the friend whom he had lost, though he tried to provoke such dreams. There were at least two occasions during the months of investigation when, after days of very vivid memories, one of the observers confidently expected to dream of a friend who had died, yet actually dreamed only of the most trivial events. Both S. and C. during the whole time dreamed of the dead very seldom and in unimportant connection with other persons. The scrutiny of these dream-records, in fact, bears out M. Délage's conclusion: "*En régle generale, les idées qui ont obsédé l'esprit pendant la veille ne reviennent pas en rêve; on ne rêve des événements importants, que lorsque l'époque où ils préoccupait l'esprit à un haut degré s'est eloignée.*"

M. Délage's examination of the facts is very satisfactory, but his theory cannot be accepted. Every image or thought, he says, has a definite amount of energy. If this force be dissipated by directing to the image or idea the waking attention and interest, then none is left by which it may be lifted above the threshold of the dream consciousness; but if, as in the case of a trivial event, the attention has been diverted from it and never rested on it exclusively, then when most presentations are barred out by sleep, when the ordinary activities of thought, judgment and attention are wholly wanting, or at least lethargic, then the trivial image or thought will rise into consciousness, and be transmitted through the dream alchemy into the rich and varied material of the dream.

Passing by the untenable Herbartian assumption that "ideas" possess an independent existence of their own, one notices at once that this theory fails to account for all the facts. It is not true that one never dreams of subjects of vital interest to oneself. The home, the family, the school, profession or business figure in all our dreams, and yet are of deepest interest to ourselves. All these subjects, however, are connected with persons, places or things of which we have frequent sense impressions. In the "professional" dreams (which form so large a class of Table XX.)

[1] Essai sur la théorie du rêve. *Revue Scientifique*, 11 Juillet, 1891.

the laboratory, the apparatus, the books, the words are the "professional" elements of the dreams and all involve sense perception. On the other hand, the persons of the dead and of the long absent, as well as the intimate thoughts, hopes and ideals of the inner life, involve the waking imagination, thought, emotion and will, but not the sense perception. The connection of the dream with the waking life, so far as it can be traced, is therefore simply this: the dream will reproduce, in general, the persons, places and events of recent sense perception or of very vivid imagination—not the objects of ordinary imagination, of thought, of emotion or of will, so far as these are not also perceived objects. Furthermore, thoughts, emotions, experiences and personal relations that mean most to us are generally extremely complex, and depend for their reproduction on the integrity of very many lines of association. When a number of these are put temporarily out of function by sleep, it is next to impossible to bring about these complicated mental states, though less complicated ones can be reproduced with tolerable completeness.

III.

Up to this point the results of this classification of actual dreams have emphasized throughout the intimate connection of the dream with the waking life, and the essentially normal character of dream-activity. Yet the characteristics which distinguish the dream from the rest of the psychic life are no less prominent. Fundamentally these are three: the comparative feebleness of the attention and the will, the want of discrimination, and the relative lack of perception. Herakleitos says that each man has his own world in sleep, but a world in common with others when awake, and Fichte describes the objective world as created by the Absolute Ego, while the dream world is the creation of the individual I. Both sayings point to the essential feature in which dreams diverge from the rest of the psychical experience, the appearance of objective reality which prevents the recognition of their subjective character. Of this phenomenon, these three explanations have been suggested: most of the sense-impressions are lacking which furnish to the waking consciousness the common test of reality; the weakness of the thought activity facilitates that wrong application of the category of causality, implicit in the false reference of a phenomenon of imagination to an external cause and in the incorrect explanation of a sense experience; the diminution of attention prevents a scrutiny of images and ideas sufficient to the discovery of their subjectivity.

There are three stages in the dream illusion. The first is that simple objectivation which we have already described, and which is characteristic of all dreams. In the next higher stage, one attributes one's own thoughts and feelings to other individuals, sometimes explicitly recognizing one's own ignorance. Of this nature is the dream which Johnson naïvely describes; he was conversing with a man who said more witty things than he. Another time-honored example is that of Van Goens, who dreamed of himself in a recitation, in which after a most painful effort, he failed, only to hear his neighbor correctly answering the same question. Maury dreamed[1] of a person who told him the location of the place Mussidan, and of another who corrected his English sentence, "I called for you," by suggesting the idiomatic "I called on you." Both these bits of information Maury, in his waking moments, did not consciously possess.

These dreams in which the dreamer figures as the ignorant hearer of what he himself does not appear to know, are the most striking examples of this dream dramatization, but every conversation dream illustrates the same illusion.[2] Besides the general occasions of all objectivation, one observes here a failure of definite recollection. A vivid speech image of one's own thought is followed by the vivid image of some person, and the two are associated through lack of definite memory of the connection of the thought with oneself.[3]

Finally, the loss of the personal identity may become explicit. One imagines oneself to be another, or one is the double of oneself, and then there is a second self whom one sees or hears. The special feature of these dreams is sometimes characterized as a loss of self-consciousness. Spitta considers that the important differentiation of dream from waking is "*die während des Schlafens eintretende Aufhebung des Selbstbewusstseins*,"[4] but he makes the Herbartian distinction between *Bewusstsein* and *Selbstbewusstsein*, and declares that "*im Traume ist nur das Bewusstsein vorhanden, das Selbstbewusstsein nicht*."[5] The fact is that explicit self-consciousness is almost always present in dreams, and that the individual ego is exceptionally prominent in the dream life. The loss of identity in dreams is not a loss, but a change or doubling of self-consciousness. It is probably correct to explain it by failure to recall the events of one's past history, or to localize them as referring to oneself. The

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[1] *Op. cit.*, p. 115, seq.
[2] Cf. dream 193, quoted, p. 328.
[3] Cf. Maury, p. 116, *op. cit.*; "phenomène de memoire."
[4] *Op. cit.*, p. 262.
[5] Pp. 138-139.

result is a loss of remembered experience which suggests changed identity. Yet all the time one is conscious *that it is oneself* who has changed, or whose identity is doubled. Images of qualities, actions or words belonging to others are simply associated with the image of oneself; or the connection with one's own life, one's name, relatives, appearance is forgotten simply; or these images are projected outward, as when one goes to one's own funeral or listens to one's own conversation—but always there is an observing self.

An example of the first sort of dream, which is often a sort of romance dream, is the following:—

Dream 43. Sunday, March 22, 6 A. M. (Observer C.) (I am at first the reader of a story; then the servant and then the sister of the hero; finally, I assume my own character.) A hero goes through great distresses * * * runs up and down long stair-cases * * * has false information given him (here, the scene is partly in Italy, looking off to the Mediterranean); finally, alter eluding pursuers, the hero plunges through a sort of tunnel in the snow, which, a guide-post warns us, is very dangerous. I, his servant, follow; we are unharmed; we realize that the guide-post was treacherous. We reach, up many stairs, the home of the hero's beloved. He and I, now his sister, enter her room; she is not there. Then we enter the dining-room. He demands her. The family shudder, and one says, "*Du hast sie.*" I realize that she has died, and cry out to him, "*Ich felicitiere dich* * * * She is yours now. Here, *eine untreue Familie* has kept her from you."[1] "It now appears that all the hero's ribs are broken. I advise him to die and join his beloved, but for some reason he refuses.—Suddenly, the hero turns into my father, whose ribs are broken, and who is questioning whether he would not better die, while I (in my own character) refuse to let him."

A frequent feature of the change in one's own identity, and of the swift transformation of one dream figure into another, is the psychical experience known as paramnesia, which is very common in dreams, though what seems paramnesia (in the waking life and in dreams) may be often the actual memory of an incident of some forgotten dream. The paramnesia of dreams appears in various forms. People or things, absolutely unrecognized by the waking self, appear in our dreams as familiar; or a familiar person is endowed with qualities—personal or intellectual—which are quite foreign to him, and well-known localities are altered in certain features. Such

[1] I think that all this speech was in German, but only the quoted words were exactly remembered.

illusions, it is evident, may be traced directly to the weakness of attention and judgment; heterogeneous images occurring in close succession are referred to the same external object. Our recorded dreams are full of instances of this sort. There are instances of it in the long dream (No. 193) quoted on page 328.

A very detailed example is recorded in the following extract from Dream 78.[1] March 29-30. Immediately before waking. (Observer S.) "- - - - - - -. When I left the chapel, I took a car. There was one track that turned off very soon to the right, the other swerved to the left and kept on up the hill. It seemed to me at first that I ought to be in a car that turned to the right, and had taken a wrong one. Then I remembered 'this is the car I have been in before, and I am all right.' As I looked back out of the car, I saw something like a galvanized iron ash-barrel rise rapidly at the end of a long iron arm, and enter the opening of an enormous tin tube - - like a reflecting telescope, thirty feet in diameter. This projected from the roof of a shed, or from between the houses. It seemed rusty and in one place somewhat broken. I remembered having been over the same line before, and to have seen the tube before. [Note.—Certainly paramnesia; never saw anything of the kind] - - - - -."

A favorite type of paramnesia, especially with one of the experimenters, was the dream of writing the records of a previous dream.

The incoherence of dreams proceeds from two main causes, one of which we have already discussed, the swift change of dream images through a process of modulated association. In this respect, the dream resembles revery, in which also the will and the judgment are in abeyance, and the subject half unconsciously makes *Ideensprunge* from one slender perch of thought to another. The second cause of incoherent dreams is effective also in revery; this is the intrusion of presentations into the chain of images and thoughts, and is a common feature also of dreams and of madness. As in the case of dreams, such presentations sometimes abruptly change the course of consciousness and sometimes are woven into its strands. So Maury tells the story of the madman on shipboard whose demon swore in Italian and made use of the real oaths of the sailors. But we may over-estimate the incoherence of dreams in our memory of them. It is easier to remember events which are in the line of our daily life than to recall the more or less fantastic or unusual train of dream-consciousness. So the gaps in some of our dreams may be breaks in memory.

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[1] A very long and "modulated" dream.

The fantastic or absurd dream to which we have just alluded is not the same as the incoherent dream, for though all incoherent dreams are in a sense absurd, these are highly fantastic and improbable dreams, which move on in stately and uninterrupted measure. The following dream is one of this kind.[1] (I quote only part of it):

Dream 157, April 22-23, 6.05 A. M. (Observer S.) "I dream of a kind of intelligent elephant that is to be executed by electricity, - - - - and which I have charge of executing. - - - - The elephant is to be fastened to the Mach (rotation) apparatus, or rather a polished edition of it in the library, or a front room otherwise nearly empty. I get things ready so far as I can, and propose to go to work the next day. - - - - The elephant objects, and by degrees I come to talk the matter over with him. Am a little surprised, at first,[2] at his talking, but remember how intelligent an animal the elephant is reported to be, and find it very convenient to talk with him (he seems to appreciate the awkwardness of my position and talks rather kindly) - - - - not maintaining that he won't smash things with all his might when he feels the current, but as yet self-controlled. - - - - "

The following table gives the number of absurd dreams counted by both experimenters among those on record. Under the head "impossible" are included only natural impossibilities, like the speaking elephant, and the skull transferred by a surgical operation to the head of a living subject, not such dreams as those of meeting with people who have died. Of "very improbable" dreams an example is the "romance-dream."[3]

TABLE XXI.

	Rather Improbable.	Very Improbable.	Impossible.	Total Absurd.
S.	29	28	5	62
C.	42	35	3	80

The absurdity of dreams is clearly due to the weakness of the judgment. When our waking imagination confronts us with impossible images, though they have the vividness of the hypnogogic illusion, we yet correctly judge that they have

[1] Cf. also dream 28 (quoted page 325).
[2] Cf. discussion of surprise in dreams (p. 339).
[3] No. 43, p. 336.

not (at least in their apparent form) objective reality. In the dream we accept all images with a passive belief in their externality. It is untrue, however, to assert, as so many do,[1] that surprise is never felt in dreams. In the elephant dream just quoted, the judgment asserted itself in the surprise with which "at first" the talking of the elephant was heard, but the wonder was soon quieted by the vividness of the impression; it is interesting to observe that reflective thought was still present in the dream, since the peculiarity of the elephant was at once explained in true natural-history style. This possibility of dream thought, active enough to explain dream-images, but not keen enough to deny their externality, has appeared, already in the skull dream,[2] in which wonder at the *insouciance* with which the remarkable piece of surgery is performed, and at the deductions drawn from it, is accompanied by the complete acceptance of the fact and by much reasonable reflection on it.

It is true, however, that we constantly witness in our dreams, without a quaver of surprise, scenes and events of the most wildly improbable character. And just as the dreamer literally does not know enough to be surprised, so also his judgment plays him false in the recognition of the beautiful. I cannot recall a single distinct waking memory of any beautiful dream object, though it is common enough to dream of something as rarely beautiful which one recognizes through the waking recollection as merely fantastic or *bizarre*. It has happened to many of us to write dream verses which have seemed to the poetizing dreamer exquisitely beautiful, but which are recognized by the waking critic as sheer doggerel.[3]

IV.

One characteristic popularly attributed to dreams is open to question. It is by no means so certain as some of us think that the train of thought and imagery in dreams is swifter than that of the waking life. This assertion needs, of course, to be fortified, for there are innumerable instances which seem to prove the alleged swiftness of dreaming. The experience of Napoleon, who dreamed of a journey, a siege and a cannonad-

[1] Cf. Spitta, p. 138. His reasoning here is particularly specious. "Die Seele," he says, "kann nicht über Etwas in Verwirrung gerathen was sie selbst geschaffen hat." But this is exactly what "die Seele" experiences in dramatic and double identity dreams. (Cf. page 335 *seq.*)

[2] Dream 28, page 325.

[3] See papers by Miss F. P. Cobbe, Macmillan's Magazine, Nov., 1870, and April, 1871; and by Bessie A. Fecklen, Scribner's Magazine, May, 1891. The last refers to a book on "Dreams and Dream Stories," by Mrs. Anna Kingsford, which contains a dream poem of eighty verses.

ing, and woke while some slight explosion was still reverberating, to find himself in the midst of the journey; and of Mauchart, who dreamed the scenes of the reign of terror, his own arrest and trial and the falling of the guillotine, and was waked by the falling of some part of the bed upon his chest, —these are only two of the stories of which there is already a large collection. Maury tells these and others,[1] and gives two proofs from his own experience of the swiftness of dreams: he has often had long dreams while reading aloud to his mother, in naps which were so short that his mother never marked a pause. His brother, while talking in his sleep, used clipped words spoken so swiftly that they were barely understood.

When one tries, however, to render to oneself any definite account of a shortening of the perception of time, it is almost impossible to do so. Tissié's remark, "*Les impressions, etant plus vives, l'ideation se fait plus rapidement,*" is not adequate, for it is only occasionally that the impressions are more vivid. It is therefore more than likely that only the memory-time, not the actual conscious process of the dream, is quickened. In the free association of the dream, the representation of one event immediately suggests that of some natural consequence, which objectively must be separated from it by a long expanse of time. But in the reproduction, after waking, of these chief links in the series of images (these *Hauptvorstellungen*), the memory adds the intervening links and one believes that one has dreamed them. The vivid images of the dream become representative of the others. "*Niemals,*" says Spitta,[2] "*ist - - - - eine Traumvorstellung genau und bis ins Detail ausgebildet.*" The retrospective lengthening of the time is more natural because the number of images, as compared with thoughts, in a dream, is always large, and the presence of many images always makes the memory-time long.[3] It is possible also that in many dreams of this sort which are narrated, the early parts may have been dreamed before the sense excitation which is supposed to have occasioned the whole dream. So Napoleon may have been dreaming of a siege and Mauchart of a trial before the explosion or before the blow, which were simply incorporated into a dream already begun.[4]

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[1] Pp. 132–134.

[2] P. 248.

[3] Spitta, p. 169. Cf. pp. 158-171, for discussion of the subject.

[4] Probably some small portion of an experience, or even a single feature of it, stands as a sort of shorthand note, from which waking consciousness develops a more elaborate account. The bare words enumerating the stages of a long experience might

A scientific discussion of dreams may profitably omit many subjects which have especially interested most of the ancient and modern writers. The question whether "the soul ever sleeps" may be relegated to the realm of metaphysics, since there is little hope of observation minute or extended enough to answer it experimentally. The importance of the dream as a revealer of character is not a suggestive topic,[1] when one has discovered that both will and judgment are relaxed or altogether lacking, and that the very subjects of one's dreams are unconnected with one's deepest interests. The nature of prevision in dreams and the explanation of so-called "prophetic" dreams, have received a certain modern interest from the labors of Myers, Gurney and others.[2] The first remark to be made is that the number and the significance of these have inevitably been exaggerated. Such dreams are seldom accurately recorded, and the after experience (the so-called fulfillment) supplies details which one then believes that one has dreamed, even when the illusion does not rise to complete paramnesia.

In the second place it may safely be asserted that a large number of these dreams admits of a perfectly natural explanation. Previsions of illness were long ago assigned by Hippokrates to real organic sensations, with which are associated images of illness. A prevailing emotion persisting in sleep is associated with the image of some event as its occasion, and in the course of time this actually does occur. Perhaps some object perceived during the day and then forgotten, determines the character of this event. Délage[3] gives an admirable instance. A friend of his, a physician, had a terribly vivid dream of the death and burial of his father. He waked and found beneath the door of his room a telegram really announcing his father's illness. He hastened away, cared for his father during a severe illness and succeeded in saving his life. The whole has all the air of a prophetic dream until one discovers that the telegram had been slipped beneath his door during the evening preceding his dream; he had seen it without attending to it and the blue envelope[4] had suggested the dream, corresponding so closely with the reality.

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serve as such signs; just the word "Hamlet" even to the waking mind recalls some of the images of the drama. This tendency of consciousness, both sleeping and waking, to coerce experience into some reasonable shape is one of the most interesting aspects of the study of dreams.

[1] See, for opposite view, Sully, Cont. Rev., March, '93.

[2] Cf. Phantasms of the Living, Gurney, etc., II, pp. 380-443.

[3] *Revue Scientifique*, 11 Juillet, 1891; Essai sur la Théorie du Rêve, p. 42.

[4] This is a Parisian telegram.

Our dream records contain five cases of dreams in which there was a certain congruity between dreams and subsequent events. One is unimportant—a dream of the arrangement of articles in a magazine, and an unexpected note on the next day, with reference to arrangement of matter, at a time when this work was engaging considerable attention. In another dream, a remark is made about some one in whom the dreamer is much interested, so incongruous with the waking opinion that the dream is completely forgotten, and is not even recalled when, a month later, the same surprising story is heard and believed. This prevision, which was very exact, would have been entirely unnoticed but for the dream-record.

The other three dreams may be quoted. They all closely anticipate coming events, but are so essentially trivial and unimportant that there is no temptation to endow them with telepathic qualities :—

Dream 95. April 4–5, 4.10 A. M. (Observer S.) "Dream of visiting - - - - Wellesley College. M. and I are shown over the buildings. Among other things I am taken to see a little rotunda where a register of visitors is kept - - - - The floor of the rotunda is black and white marble, and has a few palms or other plants in it; it is rather poorly lighted. I think of Y. M. (a former pupil) as being there, and am not sure that I do not see her in the distance."

Weeks later, an actual visit to Wellesley was made, which had not been planned at the time of the dream; a rotunda, with the black and white marble floor and the palms, was seen; and a visitors' registry was found in another building. The suggestion of the dream was probably the casual mention the day before of an acquaintance at Wellesley, but there is no remembered occasion for the close likeness of the dream scenery to the actual surroundings. Yet it certainly is easiest to suppose some forgotten picture or description recalled in the dream.

Dream 161. April 24–25. (Observer S.) "Immediately before waking, I dream of being in O. - - - - I see Dr. Y. in a reclining chair on the sidewalk - - - - I speak to him and he refers to the next "Journal," saying that he can - - - easily get up matter on ether anæsthesia - - - - ."

The next day Dr. Y. (who lives forty miles away) makes a visit and actually speaks of the "Journal." The "prevision" is very close, but it is not easy to escape the conviction that it is a mere coincidence, for the "Journal" is a common object of thought and of perception, Dr. Y. is one of its contributors; and a piece of Dr. Y.'s apparatus may have been in the mind of the experimenter at about this time.

Dream 19. Friday, March 13–14, 11.35 P. M. (Observer C.) "I am coming from Cambridge on the cars with M. M., whose aunt will meet her at Harvard square. M. is coming from Newbury street, but will not go back home. She wears a red cloak."

Street-car dreams were not uncommon at this time, for the dreamer of them was going back and forth from Cambridge several times a week. But, on March 28, a fortnight later, Miss M. was actually met on a Cambridge car. The strangest part of this correspondence of dream and reality is that Miss M. had been more than a year in Brooklyn, and was at home for only two days. But even here forgotten suggestion seems a more likely explanation than telepathy. If in a large number of dream experiences, suggested by the affairs of daily life and peopled with the larger part of one's acquaintance, there should be no coincidences, that in itself would be a most remarkable fact.

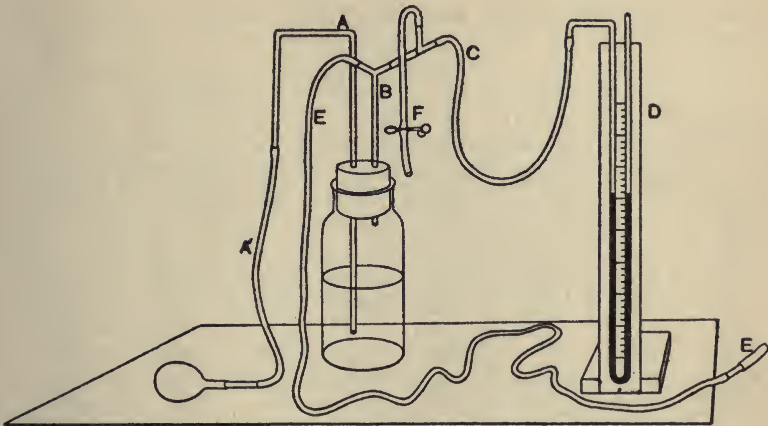
For these formulated results of the dream statistics of only two observers, no wide generality is claimed, and there are several dream experiences, of which the records show no trace, the dream, for example, that recurs always in the same form, and the dream of childhood in which the dreamer is himself a child again. Perhaps the most important effect of the whole study is the suggestion of questions which it does not answer and the indication of the useful possibilities of such study, as is in everybody's power, of the psychical phenomena of his own life. Such investigation must be slow and painstaking; it is not likely to lead to any startling results, but it cannot well fail to bring into stronger relief old truths previously accepted on hearsay, to correct traditional views, to supplement formulæ by facts, and at least to suggest some new aspect of psychological science.

ON THE PRESSURE SENSE OF THE DRUM OF THE EAR AND "FACIAL-VISION."

By F. B. Dresslar.

The purpose of this investigation was to determine in what way one is able to know when he is in the presence of objects even beyond his reach when the eyes are shut and blindfolded. This ability is especially strong with the blind, and sometimes spoken of by them as "facial vision." They not only receive much help thus in the way of protection, as is well known, but they are led to a more certain and definite knowledge of their environment than those who see are wont to think. A friend of the writer, blind from birth, could not only tell, for example, when he was approaching a forest and something of its general character, but he derived in some way much of the æsthetic feelings those who see get from sight alone. His horizon was by no means the indefinite blank homogeneity most often attributed to the blind, but it presented to him a variety approaching in a degree that of his more fortunate fellows. Much of this knowledge was due to association and inference, but much of it was gained through direct sense perception. Since the blind have no new sense, but impose on those remaining a greater task, it seemed important to determine in what degree of efficiency this capacity exists in those who see, and to analyze this sensation of "shut-in-ness" as far as possible. Prof. James says:[1] "The tympanic membrane is able to render sensible differences in pressure of the external atmosphere too slight to be felt as noise," and in a foot note says: "That the sensation in question is one of tactile rather than of acoustic sensibility would seem proved by the fact that a medical friend of the writer, both of whose *membranæ tympani* are quite normal, but one of whose ears is almost totally deaf, feels the presence and withdrawal of objects as well at one ear as at the other." In order to test this theory, a large number of experiments were made with a piece of apparatus, of which the following is a description:

[1] Psychology, Vol. II, p. 140.



Apparatus for Applying and Determining the Least Observable Amount of Atmospheric Pressure on the Tympanum of the Ear. A wide-mouthed bottle was corked with a rubber cork, which was pierced with two holes. In these holes two glass tubes were fitted, so that one (A) reached near the bottom of the bottle within, while the other (B) was much shorter. Tube (B) ended without in a double Y. Each of these openings and also the one at (A′) was fitted with a rubber tube. Tube (A′) ended in a bulb. (C) was connected with a manometer, which was fastened to an upright standard (D) and partially filled with ink. Behind this glass tube a milimeter scale was so adjusted that an observer could note the amount of the rise and fall of the liquid in the tube.

A long tube at (E) ended in a soft nozzle prepared to fit easily but closely into the external meatus of the ear. The one at (F) could be opened and closed at will to relieve any pressure caused by adjusting the tube (E) in the ear. In order, then, to determine the least perceptible amount of pressure, it was only necessary for the subject to adjust the tube in his ear, and, after the operator had restored the equilibrium, to indicate by a signal when he felt the pressure which was applied by the operator on the bulb. The height to which the column of ink was raised thus measured the pressure exerted. After each trial the subject removed the tube from his ear for a short time, then the process was repeated for ten trials.

The following table shows the averages and average variations of pressure, with F. B. D. as a subject, for a series of seven observations, each series consisting of ten trials for each

ear. These were taken irregularly for a period of a month or more:[1]

Right Ear.				Left Ear.			
No.	Height of Column in mm.	Av. Variation in mm.	Amount of Pressure in grams.	No.	Height of Column in mm.	Av. Variation in mm.	Amount of Pressure in grams.
1	33.	3.9	.32	1	50.4	1.66	.47
2	44.6	3.4	.43	2	57.2	2.	.54
3	32.8	1.84	.31	3	40.8	1.8	.38
4	38.6	1.48	.36	4	42.2	.94	.40
5	38.2	1.6	.35	5	41.	1.	.39
6	36.	2.4	.34	6	36.2	2.24	.34
7	41.8	1.44	.39	7	30.	.6	.29

Conclusions from the Above Experiments: 1. That it is a mistake to ascribe to the tympanum a very great delicacy in the perception of pressure. 2. That the sensation of being "shut in" is not due to tympanic pressure, because it is not possible for so much pressure to be exerted on the tympanum by merely approaching an object.

Suggestions: 1. The method used suggested a much better plan for determining the threshold for minimal pressure than heretofore used. It was noticed that the height to which the pressure could be raised before being felt depended on the suddenness and rapidity with which the pressure was applied. By using a regulated mechanical pressure, the variations in the threshold for pressure as affected by rate could be more clearly determined. 2. The bilateral asymmetry noticed in the above records suggests additional evidence in functional asymmetry of the two sides of the body.

Having seen that tympanic pressure could be wholly excluded as an element in the sensation, the next thing was to find a method by which the relative value of the three remaining possible elements, viz., temperature, sound and "facial vision," could be determined. This was accomplished with the following apparatus:

Apparatus and Method for Determining the Effect of Temperature, Sound and "Facial Vision." A light frame-work of wood was made four feet long and one foot wide, and divided into four spaces, each one foot square. The first one of these spaces was left open, the second latticed with strips

[1] It should be said that practically the same results as the above were obtained in the same way with two other subjects, but unfortunately the figures were lost.

three-quarters of an inch wide, and with spaces of one-half of an inch between them; the first space was closed solidly with a panel of wood, while the fourth was filled with a wire screen. The frame was then suspended from a high ceiling by four strings, fastened in pairs as guys (so that it would swing lengthwise easily and without swerving), and made to swing low enough to be opposite the face of the subject. A silk thread was fastened to the ends of the frame, and passed over small pulleys inserted in standards set some ten feet from each end of the frame. The two ends of the strings were then tied together to furnish the operator an easy and noiseless method for shifting the frame to bring the different spaces opposite the ear and face of the subject.

The method of work was as follows: The subject with closed eyes was blindfolded in such a way that little or none of the face but the eyes was covered; he was seated comfortably with his face at a distance of two or three inches from the path of the frame, and asked to judge between two spaces irregularly presented. He was required to indicate his judgment by a prearranged system of signs in order to prevent any reverberation of the voice which might vitiate the results. The first set of judgments were thus taken, and were for the purpose of finding the degree of power to distinguish between the three following pairs: Open—lattice; lattice—solid; solid—wire. The accompanying table shows the discriminative ability thus drawn out for different subjects:

Subject.	Open and Lattice.				Lattice and Solid.				Solid and Wire.			
	R.	W.	R.	W.	R.	W.	R.	W.	R.	W.	R.	W.
J. A. B.	65	15	59	25	58	2	56	0	45	0	46	2
O. C.	72	47	74	46	33	13	28	14	21	4	14	9
F. B. D.	53	24	58	17	69	1	70	4	73	0	77	2

Explanation of Table.—The figures in the columns marked *R* indicate the number of correct judgments, those in columns marked *W* incorrect. For example, when J. A. B. judged between the open and lattice spaces, he made 65 right and 15 wrong judgments in 80 presentations of the open, and 59 right and 25 wrong in 84 presentations of the lattice; while judging between lattice and solid, he made 58 right and 2 wrong judgments in 60 presentations of the lattice, and 56 right and no wrong judgments when the solid was presented, and so on for all the others.

After each set of experiments, the subjects were asked to state as nearly as possible just what they thought they had

used as a basis for discrimination. This was done for the double purpose of gaining therefrom hints as to method, and also as to the accuracy of introspection and the ability to analyze these sensations. The following answers cover the main points made:

J. A. B.—1. I could not hear the movements of the apparatus. 2. Reflected sounds formed the chief basis of judgment. Footsteps outside have different qualities of sound when the different parts are opposite the face. 3. There is a sort of a "shut-in" feeling when a closed space was presented, and a feeling of relief came when it changed to an open space. 4. Sometimes I seemed to see the frame through the side of the face, a sort of "facial vision." 5. There occasionally seemed to be a difference in temperature.

O. C.—1. I heard no sound from the moving apparatus. 2. There seemed to be a difference in temperature, and this feeling was located in the ear. 3. At times seemed to visualize. 4. Occasionally felt as if something were being pushed into my ear. 5. At times there was a feeling as if the head were being put in a box. A distinct "shut-in-ness."

F. B. D.—1. There was no sound caused by the apparatus as it moved past the face. 2. The basis for judgment seemed to be a difference in the quality of the reflected sounds. 3. When all without was very quiet, the feeling of "shut-in-ness" seemed present, and sometimes seemed localized in the temples. This localization was perhaps due to muscular tension, caused by straining the attention.[1] (This fact may in some measure account for the belief in "facial vision.") 4. Occasional differences in temperature seemed present. 5. The feeling of "shut-in-ness" arising from the nearness of the parts of the frame, is wholly different from that of direct pressure on the tympanum. 6. In later experiments, when my ears were closed with cotton, it became a difficult task to prevent my attention from wandering, because of the lack of any change in the sensation,—nothing to tie it to, as it were.

It now became necessary to devise a method of excluding the temperature and "facial vision." This was done in the following way: The ear was covered and fitted with a soft piece of cloth, in which a hole was made opposite the meatus. Then a piece of thick, soft cardboard, sufficiently large to cover the entire side of the face and neck, was cut so as to fit closely around the base of the ear to hold the cloth in place

[1] See James' Psychology, Vol. I, p. 434 f.

and shut the face from the frame. It was thought now that if the subject, after being blindfolded as before, could distinguish the separate spaces, that it would depend on differences in sound, and that the loss in discriminative ability would be due to shutting out temperature and "facial vision." The following table shows that the power of discrimination was very slightly, if at all, reduced:

SUBJECT.	SOLID AND LATTICE.				SOLID AND WIRE.				OPEN AND LATTICE.			
	R.	W.	R.	W.	R.	W.	R.	W.	R.	W.	R.	W.
J. A. B.	23	0	26	0	20	0	18	0				
O. C.	23	4	21	8	24	8	22	9				
F. B. D.	92	5	90	2	52	0	51	1	21	6	23	5

It should be taken into consideration that the cloth and cardboard on the face, despite all care to prevent, caused some little distraction of the attention, and in this way would account for some loss of discriminative ability, but a comparison of this table with the foregoing one shows very little change.

In order to test the matter more thoroughly, the face was exposed as at first, but the ears were both[1] tightly stopped, and the accompanying table shows that the power to distinguish was lost entirely:

SUBJECT.	SOLID AND LATTICE.				SOLID AND WIRE.				OPEN AND LATTICE.			
	R.	W.	R.	W.	R.	W.	R.	W.	R.	W.	R.	W.
J. A. B.	34	34	35	33								
O. C.	53	47	43	65								
F. B. D.	82	84	67	62	54	45	52	49	29	31	34	28

It was thought to be waste of time to complete this table of experiments with J. A. B. and O. C., for the obvious reason

[1]The fact that it was necessary to stop *both* ears tightly with cotton before the ability to discriminate was lost, suggests that this may account for Prof. James' medical friend's ability previously referred to.

that the previous experiments and those here with F. B. D. were conclusive.

The table proves beyond a doubt that with the subjects named, the basis of judgment was due to differences in sound.

The question arose whether or not these were wholly objective sounds, or partly subjective. This point was not fully determined, but the following observations were made: An acoustic telephone was fixed with the receiver near the frame, and the subject with the other end at some distance away. There was then no distinguishable difference.

Also it was noticed throughout all the experiments that too much noise in the room or outside was unfavorable to correct judgments, but this was perhaps chiefly due to distracted attention as well as to the general resonance of the room.

ON REACTION-TIMES WHEN THE STIMULUS IS APPLIED TO THE REACTING HAND.[1]

In his classical study of reaction-times Exner was led to the opinion that the reaction-time for electrical stimulation is longer by about 10σ when the stimulus is received in the reacting hand than when it is received in the other hand.[2] This result, as Exner himself remarks, is just the opposite of what might be expected, and, as his conclusion rests upon relatively few trials, invites a repetition of the experiment. The purpose of this short paper is to give the results of a careful repetition.

Apparatus and Methods. The reaction-times were measured with the Hipp chronoscope, set up to start at the closing of the electrical circuit and to stop at the breaking. The regularity of the chronoscope was tested with the Cattell fall-apparatus, adjusted to give an interval of not far from 130σ, but as the question to be answered was one of relative and not absolute times, it seemed unnecessary to seek an absolute correction of the chronoscope. The conditions were the same in all cases for the experiments to be compared, and the constant error remaining cannot amount to more than 10σ. The stimulus was an induction shock of moderate intensity, applied by small platinum electrodes let into the surface of the knob of the reacting key, and of a similar key upon which the finger of the passive hand rested. The stimulus was applied alternately to the end of the finger in the reacting and in the passive hand, the alternation being used to equalize practice, fatigue, strength of shock and all other conditions as exactly as possible. A Pohl commutator in the secondary circuit made it easy to shift the stimulus from one hand to the other without disarrangement of the other parts of the apparatus. Tests were made upon three subjects, S., B. and R.

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[1] The essential experiments shown in Table I. were made by Mr. J. F. Reigart. The statement of the results, however, has been left to me. E. C. S.

[2] Exner: Experimentelle Untersuchung der einfachsten psychischen Processe, Pflüger's Archiv, VII., 1873, pp. 622-623, and 655-656; also Hermann's Handbuch der Physiol.

All had worked before at reaction-time experiments, and none was ignorant of the object of the tests, but it is believed that this had little or no effect upon the result. In all cases the stimulus was preceded by a ready-signal, and the subject responded as quickly as he could. In taking the tests, Exner's own method of excluding irregular reactions was rigorously applied. The subjects, after each reaction, passed judgment upon it, calling it "good," "too late," or "too early," as the case might be. All the reactions accepted as good by the subject, and those only, were used in computing the averages.

Results. The results of the tests are given in the following tables:

TABLE I.

Stimulus alternately in the right and left hands. Subject selected the satisfactory reactions.

REACTION WITH RIGHT HAND.

	STIMULUS IN RIGHT HAND.				STIMULUS IN LEFT HAND.			
SUBJECT.	Total No. of Reactions	No. of reactions Selected.	Average.	Av. v.	Total No. of Reactions	No. of reactions Selected.	Average.	Av. v.
S.	30	10	128.4	8.	25	10	142.7	9.
B.	27	22	145.8	10.1	25	21	143.4	6.8
R.	14	11	153.3	6.6	14	13	150.0	6.1

REMARK: Subject S. seemed more critical in selecting when the stimulus was in the reacting hand. The shock seemed to him stronger in the left hand.

REACTION WITH LEFT HAND.

	STIMULUS IN RIGHT HAND.				STIMULUS IN LEFT HAND.			
Subject.	Total No. of Reactions	No. of reactions Selected.	Average.	Av. v.	Total No. of Reactions	No. of reactions Selected.	Average.	Av. v.
S.	15	10	141.9	10.9	26	10	140.2	5.8
B.	24	22	148.2	10.9	24	24	131.3	8.1
R.[1]	15	11	166.7	13.9	15	15	163.0	29.3
R.[2]	25	19	138.1	10.9	26	23	136.6	8.6

REMARK: Same remark as above for subject S. R.'s first and second series were taken a little more than a month apart.

It will be observed that the results of this table are opposed to those of Exner, except in the cases of B. and R. when reacting with the right hand, and even in those cases the excess when the stimulus was in the reacting hand is too small to be regarded.[1] In the case of S. when reacting with the right hand, and of B. when reacting with the left, the differences are larger than those found by Exner and in the contrary direction. The remark on the reactions of S., especially with reference to his greater strictness of selection when the stimulus was in the reacting hand, would at first sight seem to weaken the force of his figures. This strictness was, however, certainly not due to conscious prejudice, and may very well point to a keener sense of the difference between prompt and delayed reactions when the stimulus and reaction are on the same side. If this is the case, the point would tend to support rather than oppose the statement which the rest of the table justifies, namely, that it makes very little difference which hand receives the stimulus. The variation of the experiment giving the second half of Table I. (not tried by Exner) excludes the possibility that previous practice in reacting with the right hand should have given that hand an advantage in quickness.

The same practical equality of the reaction-times when the stimulus is given alternately right and left was shown, though by somewhat uneven figures, in an experiment made upon

[1] In order to facilitate comparison, Exner's results have been put in the form of Table I.; the full figures will be found in the place cited above, pp. 655-656.

REACTION WITH RIGHT HAND.

	Stimulus in Right Hand.				Stimulus in Left Hand.			
Subject.	Total No. of Reactions	No. of reactions Selected.	Average.	Av. v.	Total No. of Reactions	No. of reactions Selected.	Average.	Av. v.
E.	8	6	139.0	4.1	17	11	128.3	5.4
v. B.	12	4	146.9	3.7	12	3	138.1	4.3

The subject E. was Exner himself; the subject v. B. had never engaged in such experiments before. In the case of v. B. some of the reactions were called *ziemlich gut* or *nahezu gut*; these Exner excluded from the averages, as also two reactions that were called good by the subject, but were considerably larger than the rest.

another subject and at an earlier time by Dr. Sanford.[1] In this experiment a genuine touch with a hard rubber point was used as the stimulus instead of an electric shock. The result is given in Table II.

[1]The experiment here referred to was made with a number of others soon after the opening of the University, and before the full equipment of the laboratory. The times were measured with the Hipp, but the battery used was not of the most constant kind and the regulating apparatus was the antiquated Hipp ball-dropper. The results given in Table II., however, are regarded as valid for purposes of comparison between right and left, for the stimuli were alternately applied and both quantities may be assumed to have been equally affected by what changes occurred. At the same period experiments were tried with electrical stimulation of the finger on each side, but it was difficult to secure uniform intensity of shock and for that reason less confidence is felt in them. They were made upon three subjects, with some variety of conditions. With two of these the differences were small and now in one direction, now in the other. With the third—the least experienced and probably, also, the least informed as to the purpose of the experiment—part of the figures agree in direction with those of Exner, as appears in the following little table. The figures given are those for averages, corrected as in Table II., but in no case does the correction alter the direction of the difference.

In the first, third and fifth series the results for this subject agree in nature with those of Exner.

	Stimulus Right.		Stimulus Left.	
	Average.	Av. v.	Average.	Av. v.
Reaction with right hand, pressing key,	155.3	11.7	146.6	9.9
	147.9	12.3	147.0	10.5
	144.7	20.1	130.0	10.8
Reaction with left hand, pressing key,	173.1	13.5	166.0	17.3
Reaction with right hand, withdrawing finger from key,	183.4	15.1	178.8	11.4

TABLE II.

Stimulation by contact, alternately right and left. Stimulus applied in the first set to the back of the hand; in the second set to the second joint of the thumb. Reaction in the first set by pressing a key with finger of right hand; in the second set by removing finger from the key. Subject A.

	STIMULUS IN RIGHT HAND.			STIMULUS IN LEFT HAND.		
Subject.	No. of Tests.	Average.	Av. v.	No. of Tests.	Average.	Av. v.
A [1]	25	118.4	21.7	25	132.4	19.0
	22	**113.9**	**13.8**	**22**	**127.7**	**13.7**
A [2]	24	141.9	19.4	24	139.7	19.9
	21	**135.7**	**13.7**	**21**	**136.4**	**16.0**

The corrected results in this table, indicated by heavy faced type, were reached by rejecting from the corrected averages the three reaction-times most variant from the original uncorrected average.

Of the same tenor is the result of an experiment upon R., when no selection by the subject was attempted. The stimulus was applied alternately right and left; the reaction was with the right hand. When the stimulus was in the reacting hand, 22 tests gave a mean of 124.4σ with an average variation of 8σ; when in the other hand, a mean of 140σ with an average variation of 7σ. Casting out from each 22 the three reactions most variant from the mean, the remaining 19 average respectively 124σ and 140σ, and the average variations are 6σ and 5σ.

With this agree also some measurements made by Dumreicher,[1] though his point was the discovery of the conditions giving the greatest uniformity of results rather than the greatest quickness. He found for reaction by withdrawing the finger from the key (the stimulus was given through electrodes in the knob of the key) 126σ when the stimulus was in the reacting hand and 158σ when in the other hand; and for reaction by pressing the key, 173σ and 187σ respectively. His measurements were made with Ewald's chronoscope.

[1] Zur Messung der Reactionszeit, Inaug. Dis., Strasbourg; pp. 43, 44.

EXPERIMENTS UPON PHYSIOLOGICAL MEMORY BY MEANS OF THE INTERFERENCE OF ASSOCIATIONS.

By John A. Bergström.

The interference of associations is a fundamental fact of the nervous system and so of frequent occurrence. In learning a new way of playing an instrument, or a second system of shorthand, or a new style of writing, or in substituting a new movement in gymnastics, or any game of skill; in general, in changing from an accustomed way of doing a thing to a new way, the old habit resists displacement; and if we act hastily, or are fatigued or absorbed in something else, we are liable to act in the old way. It is not evident, however, without experiment that the old habit interferes with the formation of the new one. It is easier to act in the old way, but it is not evident that we cannot learn a new way, if we try, as easily as we learned the old one. The reason we follow the old way may be a disinclination to effort. In the small field of the present experiments it will appear, however, that in spite of every effort a very decided interference takes place when we attempt to associate a new reaction with an old stimulus, and that the interference is so constant that it may be made a means of measuring memory just as the greater ease in relearning something once learned has been used in other experiments. The confusion which results in the minds of young students in presenting a subject to them in more than one way is an illustration in another field. Much that has been attributed to the decreasing of memory power with age, as in the case of learning languages, should, perhaps, come under the category of interference. Interference, as well as practice, should be taken into account in all psychological experiments when the same stimuli or signs are used in different relations.

Methods and First Experiments. The method to be described was developed in studying the errors of an experiment upon quite another problem. In this, use was made of a test based upon the method of finding distinction and choice

times by means of an ordinary pack of playing cards.[1] Unprinted cards with the best slipping qualities were procured and made up into packs of 80 each. There were ten kinds of cards in each pack, each kind marked by an abstract word printed at the top. The cards were well shuffled and sorted as rapidly as possible into ten piles, each pile having all the cards with a given word. Chance determined the order of the piles, and this was in the main different in successive experiments. Each experiment consisted of sorting in quick succession two packs bearing the same words into different piles, which we may represent by letters as follows:

1.					2.				
A	*B*	*C*	*D*	*E*	*G*	*J*	*H*	*A*	*F*
F	*G*	*H*	*I*	*J*	*I*	*C*	*E*	*B*	*D*

In sorting the second pack the time was longer and the subject appeared confused, and had a tendency to place the cards in the positions they had had in the first half of the experiment. Two different reactions were associated with the same sensory stimulus and interference took place. The experiment was not directed to the study of this phenomenon, and the indirect results we obtained simply established the fact of interference. During the last of March and the first of April, '92, eight to ten experiments a day were made at regular intervals for from three to eight days in the case of the following persons. The first column gives the average time in seconds for sorting the first pack, and the second the excess of time for the second pack:

	1.	2.
W. L. B.,	158.7	17.5
L. L. B.,	122.5	10.1
A. F.,	127.8	18.
E. N. B.,	158.	16.8
W. O. K.,	145.9	3.5

When the second pack bore different words from the first there was practically no interference, as appears in the results for another subject, F. B. D.

The average of nine experiments on nine different days upon F. B. D. gave a difference of 30.25 seconds when the first and second packs were alike; and for three days in which two packs with different words were sorted successively, a difference of only 0.53 seconds. The pack with different words was procured to avoid the interference due to mak-

[1] Science, VIII., 1886, 238.

ing the first associations. If the words were different in the two packs and the retardation took place nevertheless, a part of it might be supposed to be due to fatigue. If no lengthening of time occurred, then the differences in the experiments with the packs having the same words would all be due to the interference of the associations. The decision of this point, however, must be left to the direct experiments presently to be described, since the evidence from these early indirect ones is incomplete. In 23 and 30 experiments respectively with A. F. and W. O. K., a second pack, different from the first, was sorted more quickly by 2.7 and 3.1 seconds. A part of this difference may be due to a difference in the packs, since, unfortunately, the order in which they were taken was not changed to make compensation.

Later Experiments. In order that the interference of associations might be under favorable experimental conditions, the sensory and mechanical parts of the process were simplified as much as possible. Instead of words, pictures of common objects were drawn at the top of the cards, and care was taken to have every picture different from every other. One set, for example, consisted of a house, a book, a coffee-pot, an acorn, a palette, a clothes brush, a glass, a safety-pin, a fan, and an oak leaf. They must be easily distinguished and not liable to grouping or other mnemonical devices; and all the pictures in all the sets must be different. The cards were held face up, and thrown upon a table covered with a rough white cloth to keep them from sliding.

The first problem studied was the rate of decrease of the interference with increasing intervals of time between the first and second sortings. An experiment consisted of two parts: the first, the sorting of a pack of cards into piles in given positions; the second, the sorting, after a certain interval of time counted from the end of the first to the beginning of the second parts, of a pack having the same pictures into piles in entirely different positions. Seven, and in one case eight, different intervals were used ranging from three to 960 seconds. One experiment a day for each interval was taken at a given hour. This required eight double sets of cards, the actual number of which was reduced one-half by having pictures at both ends of the cards. A complete record of the positions occupied by the cards from day to day was kept so that no repetitions should occur till after several days. Since a card can occupy any one of the nine other positions than the one in which it first occurs, there are ten arrangements possible without a single repetition. It would probably make the results more regular to have a sufficient number of sets to make any repetition whatever unnecessary. The relative

results sought in this case should, however, be nearly the same, since all the packs received approximately the same treatment, though we may expect to find the probable errors made somewhat larger thereby. Two minutes' rest was allowed between the experiments on different intervals, and all together they required nearly an hour a day. To allow for practice, fatigue and changing conditions in the hours, the experiments were begun with the different intervals in turn. The cards were usually picked up from the table irregularly, but sometimes shuffled, and in either case care was taken that no two cards with the same pictures should come together and that there should be no recurrence of orders in the packs. The results obtained appear in the following tables:

Table I. Subject, F. D.; member of the Pedagogical Department; unpracticed in this kind of experimental work; not aware of the purpose of the experiment till the end; refrained from any speculations about the matter. In sorting the first pack, the ability to throw the cards without looking at the piles was reached in some cases; usually all but two or three positions were known. There was no tendency to employ mnemonical schemes. In the second sorting, false movements were very numerous at first; some tendency to employ mnemonics; so strong a habit of looking towards the former positions that cards directly in front were often hard to recognize. The records for the first four days were only partial and very irregular, and so were not used in making out the average. The rest number 16 days, or 112 experiments. The first horizontal row of figures in all the tables gives the intervals between the first and second packs. The second and third horizontal rows of figures, with the exception of the first and last figures, give the corresponding difference between the sorting of the first and second packs, each figure being the average of eight days' records. Next comes the average of the differences, then the probable error of these averages. If we assume that at each interval the same quantity is measured, and that the variations are due to accidental errors, Chauvenet's criterion for the rejection of observations may be applied. The result is given in the last row. The first figures at the left (Av. P.') give the average of all the first packs for the time indicated, and so show the result of practice. The figures at the right (Av. D.) give the average of the differences, which, in this case, show a slight increase rather than decrease with practice. The condition of making the greatest possible haste was not strictly complied with, since the subject had a tendency to select the rate he thought would enable him to make the best time. All figures represent seconds. The averages of the differences from Table

I. are shown in graphic form in Fig. 1 of the diagram at the end of the paper.

TABLE I.

	(Av. P.′)	3″	15″	30″	60″	120″	240″	480″	Av. D.
Oct. 25-Nov. 2.	80.52	26.2	23.3	17.93	17.16	20.03	14.4	13.06	18.87
Nov. 3-12.	72.62	27.03	19.88	23.45	25.	20.18	16.35	14.06	20.67
Average of Dif.		26.62	21.59	20.69	21.08	20.11	15.88	13.56	
P. E. of Av.		1.23	1.25	1.50	1.45	1.18	.96	.97	
Corrected by Chauv. Crit.		26.64	22.64	22.02	21.8	19.10	16.25	15.29	

Table II. Subject, O. C.; member of the Pedagogical Department; unpracticed in this kind of work, and also unacquainted with the purpose of the experiments. As in the first case the first four days are omitted in making up the

TABLE II.

	(Av. P.′)	3″	15″	30″	60″	120″	240″	480″	Av. D.
Oct. 24–Nov. 2.	67.59	18.46	15.33	15.83	14.39	13.31	13.72	14.28	15.05
Nov. 3-12.	63.72	16.2	11.63	12.54	12.31	8.00	9.72	8.42	11.29
Average of Dif.		17.33	13.48	14.18	13.45	10.66	11.72	11.34	
P. E. of Av.		.91	.74	1.17	1.26	1.26	1.25	1.18	
Corrected Results.		17.39	13.48	12.42	12.2	9.14	10.2	9.27	

table. Eighteen days' records remain — 126 experiments. The averages in the second and third horizontal rows are made on the basis of nine records for each. Here also the tendency to regulate the speed so as to make the best time

was noticed. The eyes were very active, following the throwing of each card, as far as the experimenter could judge. The nice arrangement of the piles would show the same. The differences are comparatively small, and as is seen in column (Av. D.), there is a considerable decrease in the average of the differences. The constant use of the eyes did not permit so strong a habit to be formed; and their quick motions probably indicated an individual peculiarity of temperament and muscular control, though these may be dependent upon the absence of strong tendencies in the directions of the previous positions, which seems to the writer most probable. The averages of the differences from Table II. are shown in Fig. 2 of the diagram.

Table III. Subject, M. E. B.; acquainted with this kind of work from acting as experimenter in the experiments of Table IV.; also trained in sorting other cards than those used. All the records taken are made use of in the table, since there was no need of allowing for practice in the manipulation of the cards. By watching the eyes of the subject, the time when all the positions were learned could be noted down. The average of this time for the last three days is 34 seconds for the first pack; the last was rarely learned entirely, even with the longer intervals, and then only after 65 or 70 seconds. The greatest possible speed was attempted, and there was no conscious adjustment of the speed to make in the judgment of the subject the quickest time. There are 12 days' experiments in all—94 records. The 960″ interval has only 10 records. The averages in the second and third rows are based on six records each, except as just stated, those of 960″ interval. The averages of the differences from Table III. give Fig. 3 of the diagram.

TABLE III.

	(Av. P.′)	3″	15″	30″	60″	120″	240″	480″	960″	Av. D.
Nov. 8-13.	60.08	29.2	23.66	17.23	19.03	13.71	12.83	11.2	10.22	17.11
Nov. 14-20.	59.37	28.38	23.65	22.55	15.66	13.43	13.36	8.96	5.36	16.42
Average of Dif.		28.79	23.66	19.89	17.34	13.57	13.09	10.08	7.79	
P. E. of Av.		.70	.92	1.20	1.01	1.31	1.23	1.62	1.11	
Corrected Results.		28.79	23.66	21.16	18.42	12.26	13.09	10.08	7.79	

Table IV. Subject, the writer. During all but the first seven days of the experiment the subject was engaged two hours a day with experiments upon others. Every effort at speed was made, and there was no conscious allowance for what might be the best rate. The records were taken between 7 and 8 A. M. No records are omitted to allow for practice; but three days' records are omitted in the latter part of the time, because of some irregularities in health and sleep, which made the circumstances abnormal. The average, with these included, is given below for comparison. The table is based on 21 days' records—147 experiments. The averages of the differences from Table IV. give Fig. 4 of the diagram.

TABLE IV.

	(Av. P.′)	3″	15″	30″	60″	120″	240″	480″	Av. D.
Oct. 13-20.	84.15	31.07	26.71	16.69	12.19	14.28	11.29	8.71	17.37
Oct. 21-31.	65.68	35.86	27.07	25.60	19.57	11.54	12.29	13.39	20.76
Nov. 1-9.	61.98	31.71	21.76	18.97	15.53	15.43	12.56	11.03	18.14
Average of Dif.		33.09	25.18	20.42	15.76	13.75	12.04	11.04	
P. E. of Av.		1.20	1.07	1.10	.83	.81	.65	1.04	
Corrected Results.		32.15	25.18	19.50	15.76	13.75	12.00	11.04	
Ave. of all 24 Records.		32.65	25.75	20.08	16.3	14.94	12.08	11.54	

Table V. These records, though taken first, are placed last, because the arrangement of the experiment is different. Two experiments were taken every two hours, between 8 A. M. and 6 P. M. The cards with words instead of pictures were used. The experiments were so distributed that two records at each time of the day were to be taken for each interval. It was hoped by this means to find not only the rate of disappearance of neural memory in general, but also the difference for different times of day. The plan of the experiment is open to objections on other grounds, but the fact that the same cards were used incessantly, and that only two hours

were allowed for the habit to decrease in intensity before another trial was made, would lead us to expect great irregularities. The subject was T. L. B.; familiar with psychological experiments, and in good practice with the cards; acquainted in a general way with the purpose of the work. To Table V. corresponds Fig. 5 of the diagram.

TABLE V.

	(Av. P.')	3″	15″	30″	60″	120″	240″	480″
June 1-13.	80.86	21.85	21.42	17.88	12.25	10.02	11.2	10.25
P. E. of Av.		2.28	2.19	1.87	2.14	2.02	3.96	1.44

There are 66 experiments in all; 10 or 11 experiments each for all, except the last two intervals, for which there are only 7 and 4 respectively.

These differences might, perhaps, be supposed to be due in large measure to fatigue. To test the effect of fatigue or practice, some experiments were made on successive days, as before, in which two packs with different pictures were sorted successively with the shortest interval, where fatigue would show itself if anywhere. Half the experiments began with one, half with the other pack, to compensate for any differences in them. The averages here as in the other experiments are in seconds. P^1 and P^2 are the first and second packs respectively. The first two averages include 12, the last two 16 experiments each.

TABLE VI.

	P^1	P^2	$P^1 - P^2$
O. C.	63.53	62.94	.59
F. D.	66.8	65.37	1.43
M. E. B.	56.62	56.49	.13
J. A. B.	64.61.	63.14	1.47
Average.	62.89	61.99	.90

Thus when there is no chance for interference to take place the tendency is in all cases to sort the second pack a little quicker than the first. We may conclude, therefore, that the differences are not due to fatigue.

The attitude of the subject is here the reverse of that in the work of previous experimenters. In their experiments, his interest was to remember, in these to forget. The influence of the knowledge that something is to be remembered upon its retention is well known. The relatively large amount of interference throughout the series, and the slight diminution of the average of the differences, as seen in the right-hand column (Av. D.), makes it probable that forgetting—that is, the disappearance of habit—is not an active process of disintegration, but merely a letting alone. The rapid rate of disappearance at first was one of the most striking facts noticed in the preliminary experiments.

The uncorrected average differences, and their average for all the subjects, are given together for the sake of comparison in Table VII.

TABLE VII.

	3″	15″	30″	60″	120″	240″	480″	960″
F. D.	26.62	21.59	20.69	21.08	20.11	15.88	13.56	
O. C.	17.33	13.48	14.18	13.45	10.66	11.72	11.34	
M. E. B.	28.79	23.66	19.89	17.34	13.57	13.09	10.08	7.79
J. A. B.	33.09	25.18	20.42	15.76	13.75	12.	11.04	
T. L. B.	21.85	21.42	17.88	12.25	10.02	11.2	10.25	
Average.	25.54	21.07	18.61	15.98	13.62	12.78	11.25	[7.79]

Reference to the results corrected by Chauvenet's criterion shows that those of F. D. and O. C. follow the average in general more nearly than the uncorrected results; but the assumption on which the corrections rest is at best only approximately true. The average difference between the 3-second and the 8-minute differences is 14.28 seconds; and between the 1-minute and the 8-minute differences 4.72, which

shows that about $\frac{2}{3}$ of the decrease took place in the first minute. Indeed, references to the tables will show that we can hardly be sure of any decrease from 2 to 8 minutes with the few observations we have. The heightened form of neural dispositions, due to immediate intense activity, drops quickly to a low level, from which the decrease is comparatively slow. The attention is in this case turned away completely from the first positions, since the subject's interest is to forget rather than remember them. In 24 hours the subject can sort the cards as rapidly as at first. This, however, does not mean that the neural disposition has vanished. The experiment consists rather in raising a habit temporarily above the rest, and noting its interference with another raised similarly above the same base of opposition. The difference at 3 seconds does not measure the strength of the habit at 3 seconds from the time of sorting the first pack, but is relatively too small, since the habit is found to decrease rapidly during the process of sorting the second pack. A considerably weaker habit applied uniformly as an interference might have produced the difference. Only an estimate can be made as to how many seconds of work with the second pack would have had to elapse before this average habit would have been reached. If we put it at 30 for the 3-second interval, it would be nearly 45 for the 15-second interval; and the decrease in the differences would be between these two points. But after about a minute and a half, when the great decrease in the differences is at an end, the method measures accurately the rate of forgetting.

A tentative analysis of the process may be permitted here, as it will bring out some of the essential conditions of the experiment. In sorting the first pack, two of the subjects learned the positions during the placing of about a third of the eighty cards, so that the process could go on without further use of the eye to find the place for, or to verify, each throw. After a little practice there was very little feeling of effort, and the process was largely mechanical. It takes on the average all the subjects when in practice 65 seconds to sort the first pack; to sort the second immediately afterwards requires, however, about 85 seconds, and numerous false motions in the direction of the first positions are made. The false movements, and the errors which the subject was obliged to correct, and the consequent retardation, show that a strong association has been formed. At first the eye is moved in the direction any card is to occupy. The movement of the arm in placing the card may follow the eye as a reflex or as a separate act of the will. In the first case, in which we have supposed the arm to follow the movement of the eye reflexly, the association which

interferes would be between the visual centres receiving the image of the picture and the centres moving the eye. As soon as the picture was seen, the eye would snap in the direction of the strongest habit, or toward the former positions, and the arm would follow and make the visible false motion, not because of an association between its centres and the visual centre, but because it is in reflex *rapport* with the eye. In the second case, the association would be formed between the image of the picture and the arm movement, and the eye movement might be unimportant in the production of the interference; or the picture may be the signal for sending an impulse both to the eye and the arm at the same time. The localization of the interference is an important psychological question. It is a mechanical struggle of habits, and might at first be supposed to be very definitely localized, so that the association of the positions with the motor organs of speech would not interfere with sorting by the hand. In nine experiments, in which the positions were learned from seeing the cards on the table by repetitions, like those employed by Ebbinghaus with the nonsense syllables, an interference of 30.02 seconds took place, as compared with sorting a pack, without such learning, immediately before and after each series of experiments. In another series the positions were learned, not by looking at the cards, but by being told where each one was, till they were learned. It was supposed that by using neither the eye nor the arm, but getting the information by another sense, and expressing it with the vocal organs, interference might be avoided for the sorting when the eye and arm were the organs used. The interference in this case was 23.25 seconds, as the average of nine experiments. The positions were learned in each case, till they could be repeated forwards and backwards. The interference is considerably less when the ear was the receiving sense, but the experiments are too few to make definite comparisons. The whole matter requires additional experimental study. As far as these experiments go, they show that it makes little difference what sense is the receiving sense, or what organ the reacting organ, as regards the production of interference for the sorting by hand. The information gained by one sense is perhaps not confined to the centres of that sense, but awakens the other senses whose special data have contributed, or may contribute, to the perception of the object—the eye visualizing, the arm moving, or tending to move, as if it were the organ used.

In sorting the cards quickly, the mental process is not an intense form of the process that would take place if the sorting were done slowly. In the latter case no false motions

need be made, since the subject would know that no position of the first stage of the experiment was repeated in the second, and if he made a mistake, he would learn from experience, and not repeat it. Only a very small part of such complete consciousness is present in rapid sorting, since not only are errors made once, but the same error is repeated many times, especially with the shorter intervals. When the effort is to make the greatest possible speed, we produce the condition of automatism voluntarily. We have in this a laboratory method of studying habits.

An illustration of interference is found in the familiar game in which a person is asked to repeat sentences like the following as fast as he can: "Shall he sell sea shells? Shall she sell sea shells?" "Shoes and socks shock Susan," or, "Peter Piper picked a peck of pickled peppers," etc. Great difficulty is experienced. In the first half of the first sentence the simple *s* sound is repeated twice in "sell" and "sea;" it begins "shells," and if the person repeats very rapidly, so as to exclude the higher mental processes, the habit formed by simply repeating the *s* sound twice will make him say "sells" for "shells." Similarly the repetition of *sh* twice in the second half will probably make him say, "shall she shell." Every one is familiar with similar errors in writing, where words beginning with the same letters are liable to confusion. This is especially noticeable in fatigue, or cases of abstraction and haste. The cards provide an easy means of variations, so that the phenomena can be studied quantitatively. After considerable practice, the visible number of false moves diminishes, but as will be seen in the tables, the average retardation does not grow very much less. A habit of hesitation has been formed, but does not lessen the interference very much, as measured by the increase in time.

Historical.

An extended account of the experiments of others on memory is not needed here, Dr. Burnham's study in the second volume of this *Journal* being sufficiently accessible. A few papers, however, have been published since Dr. Burnham wrote,[1] and some of them have more or less relation to the experiments of this paper.

[1] Paneth: Versuche über den Zeitlichen Verlauf des Gedächtnissbildes. Centralblatt f. Physiol., Bd. IV., No. 3, 1890.
Schumann: Ueber das Gedächtniss für Komplexe regelmässig aufeinander folgender, gleicher Schalleindrücke. Zeitschr. f. Psychol., Bd. I., 1890.
Schumann: Ueber Contrasterscheinungen in Folge von Einstel-

Müller and Schumann give an account of some interesting and important observations on what they call motor adjustments. If a heavy weight is lifted a number of times, a habit of expending a given amount of energy is formed, and lighter weights lifted afterwards will appear lighter than they ordinarily do. The method of experimenting is first to find a comparison weight heavier than that which can just be distinguished from the standard each time; then the heavy weight is lifted in place of the comparison weight several times. On returning to the comparison weight, it can no longer be distinguished, and may even be judged lighter than the standard. The influence of the motor adjustment was noticed even after twenty-four hours. The phenomenon is thought to be due, not to contrast, but to the formation of the motor habit, and the illusion takes place because the comparison weight is lifted with greater velocity than usual. Dr. Schumann has given an account of similar phenomena in another field. The normal rate of letters on a revolving drum seemed slower or faster than usual, after a short habituation to faster or slower velocities respectively. Vigor and fatigue seem to have a somewhat similar effect. Distances traced by the hand were misjudged after habituation to other distances. These phenomena, which resemble the "interference" of this paper, are very important for psycho-physical experiments as well as for a study of the nervous system.

Dr. Münsterberg has studied the interesting theoretical question whether a habit associated with a given sensory stimulus can continue automatically, while some effect of a previous and different habit associated with the same stimulus remains. He believes the question can only be studied when the attention is entirely distracted from the experiments, and therefore it is to be studied in the actions of daily life, not in the laboratory. The experiments were made with his inkstand, his watch, and the doors of his room. In each case a given habit is practiced (in the course of daily life—not specially) till it becomes automatic—as, for example, taking his watch out of his pocket on the left side. A different habit is then practiced; in the case of the watch, of taking it out of the

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lung. Nachrichten v. d. k. Ges. d. Wiss. zu Göttingen, Dec. 3, 1889. No. 20.

Müller and Schumann: Ueber die psychologischen Grundlagen der Vergleichung gehobener Gewichte. Pflüger's Archiv, Bd. XLV., 1889, pp. 37-112.

Münsterberg: Gedächtnisstudien. Beiträge, Heft 4, 1892.

Cattell and Fullerton: On the Perception of Small Differences. Publications of the Univ. of Penna. Philos. Series, No. 2, 1892, pp. 147-149.

pocket on the right side till this is done automatically. He then returns to the old habit, and finds that it takes less time to relearn this than it did to learn the second, and therefore concludes that some effect of it remained, although the second was automatic. If this process is repeated several times with the same object, the time required for relearning each of the two habits grows less and less. This shows, he believes, that the habits are gradually being developed to their maximum strength, and that only a little difference one way or the other is sufficient to make the habit do its work automatically, and that nerve currents do not behave like electric currents, which divide into several conductors inversely as the resistance.

It only remains for me to express my obligation to those that have assisted me as subjects, and in particular to my wife, whose work both as a subject and assistant has contributed much to the success of the research.

A NEW INSTRUMENT FOR WEBER'S LAW; WITH INDICATIONS OF A LAW OF SENSE MEMORY.

By James H. Leuba.

The three methods generally used in the study of the relation between physical stimuli and the sensations they produce—the Method of Least Observable Difference; the Method of Average Error; the Method of Right and Wrong Cases—are only different ways of reaching the same end, viz., the determination of the least observable difference in sensations.

Delbœuf and Wundt have attempted to verify Weber's Law by following another road. Instead of seeking to measure the least preceptible increment of sensation at different points of the scale of intensities, they seek to determine a middle sensation between two others, varied at will. This Method of Equal Intervals *(Methode der mittleren Abstufungen)* admits of a modification of some importance; it consists in the classification between two limits of a series of sensations into groups, in such a way that each group will appear to be at an equal interval from the one immediately preceding, and from the one immediately following it.

The comparison of the magnitudes of the stars, as determined by the eye, and recorded in the various catalogues, with the photometric measurements of their light intensities, is an application of this method, if, indeed, we can assume that the sole desire of the star-catalogue makers was to classify the stars into magnitudes, each appearing equally distant from the next.[1]

This method is evidently capable of wide and useful application, equally well in the measurement of intensities as in that of extensity. Prof. James, contrasting the Method of Equal Intervals, of which the Classification Method is a species, with the three usual methods, says: "At first sight there seems to be no direct logical connection between this method and the preceding ones. By them we compare equally

[1]The last comparison of this kind was made by Prof. Jastrow. See *Amer. Journal of Psychology*, Vol. I. p. 112.

perceptible increments of stimulus in different regions of the latter's scale, but by the fourth method we compare increments, which strike us as equally big. But what we can but just notice as an increment, need not appear always of the same bigness after it is noticed. On the contrary, it will appear much bigger when we are dealing with stimuli that are already large."

If the relation of the sensations to the stimuli producing them is found by the Method of Equal Interval (or the Classification Method) to be the same as when established by one of the Methods of the Least Observable Difference, the just perceptible increment in sensations will have been proved to remain equally big in the observer's consciousness. This question is in itself of sufficient interest to warrant experimentation with the Interval Methods.

Prof. Jastrow is, so far as we know, the only person who has made use of the Classification Method. He applied it in the study of star-magnitudes, just mentioned, and in experiments on the spatial relations of vision, on the tactile-motor sensations, on the time-sense, and on the motor-sense.[1]

In order to adapt this psycho-physic method to demonstrational purposes, Dr. Sanford last year devised an instrument (Fig. 1) for the production and the measurement of artificial stars of different magnitudes, using the principle of the episkotister for regulating the amount of light passing through a minute hole in a metal plate.

The present paper records the experiments made with this device, according to the Classification Method, to test the possible usefulness of the instrument.

Two lots of results were obtained, one with the apparatus just mentioned (Fig. 1), the other with a modification of it, suggested by the writer (Fig. 2).

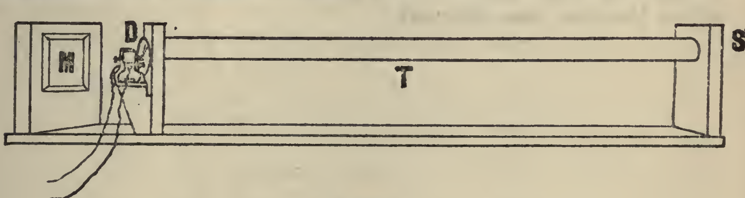

FIG. 1.

The most important part of the first apparatus is the episkotister, two equal discs (D), four inches in diameter, set on the axis of a small electric motor. Each of them has two openings of 90°, in opposite quadrants. The discs can be so placed in relation to each other that their openings will coincide on

[1] See *American Journal of Psychology*, III. and IV.

any angle between 0° and 90°. To permit of exact adjustments, one of them is graduated along one of its openings. A light thrown on the mirror M, is reflected towards the discs, and as they rotate, a quantity of light proportional to the opening passes through to a very small hole, bored in metal, at the extremity of the tube T, and covered over on both sides by pieces of glass to keep it free from dust. The subject, seated at the other end of the tube and looking in at S, perceives the light in the form of a star. The position of the observer can be fixed by reducing the opening of the tube so that he will have to sit with one eye close to the small space left free in order to see the light. The tube T is about six feet long. By means of a switch, the motor can be readily stopped for the readjustment of the discs.

The two extreme stars used as standards were made by an opening of 180° and 10° respectively. The latter is barely perceptible; the former is about the size of the smallest stars of the first magnitude. At the beginning of a new sitting, the subject was shown the two standards, and also during its course, whenever he desired to see them again, though this was rarely asked for. In each series of experiments the subject was shown the forty stars made by the forty openings, between 10° and 180°, which are given in degrees in the degree columns of Table I. The series was so chosen that about the same number of stars would fall into each of the five classes if the psycho-physic law were followed. They were shown in an irregular order, and the subject was requested to group them in five classes, or magnitudes, endeavoring to make the differences between the classes equal. His answers were recorded in tables similar to Table I., opposite the figures in the first column representing the measure of the intensities of the lights. The numerals stand for the class to which the star was referred.

TABLE I.

Degrees.	1st Series.	2d Series.	3d Series.	4th Series.	5th Series.	Degrees.	1st Series.	2d Series.	3d Series.	4th Series.	5th Series.	Degrees.	1st Series.	2d Series.	3d Series.	4th Series.	5th Series.	Degrees.	1st Series.	2d Series.	3d Series.	4th Series.	5th Series.	Degrees.	1st Series.	2d Series.	3d Series.	4th Series.	5th Series.
180	1	1	1	1	1	112	2	2	2	1	2	64	2	3	2	3	2	32	3	3	4	4	3	16	3	4	4	5	5
170	1	1	2	1	1	106	2	2	2	1	2	60	2	3	3	2	2	30	4	3	4	4	3	16	4	3	5	5	3
160	1	1	1	1	1	100	3	1	1	1	1	56	3	2	2	3	2	28	4	4	3	3	5	14	4	4	4	4	3
152	2	1	1	1	1	94	1	2	2	1	1	52	2	1	2	3	2	26	4	3	4	4	4	14	5	4	5	5	5
144	1	2	1	2	1	88	3	2	2	3	3	48	4	3	3	3	2	24	3	3	3	4	5	12	5	5	5	5	5
136	1	1	1	1	1	82	1	2	2	1	1	44	2	2	2	2	4	22	4	4	5	5	2	12	4	5	5	5	5
128	2	1	1	1	1	76	2	3	2	3	1	40	3	2	3	5	3	20	3	4	3	4	4	10	5	5	5	5	5
120	2	1	2	2	1	70	3	3	2	1	2	36	3	2	3	2	3	18	4	4	3	4	4	10	5	5	5	5	5

Table II. summarizes Table I., giving the average of each class in each of the five series, and, at the bottom, the average ratios between the classes. The subject has attempted to place his sensations in an arithmetical series of five terms; we have here the objective measurement of the stimuli which produced these sensations; if Weber's Law were exactly followed, the ratios should be equal.

TABLE II.—CLASSES.

SERIES.	I.	II.	III.	IV.	V.
1st.	138. (7)	91.4 (10)	48.2 (10)	23.7 (9)	11.5 (4)
2d.	133.1 (9)	80.2 (10)	44.6 (10)	18.8 (7)	11. (4)
3d.	142.8 (7)	87.2 (13)	34.2 (8)	26. (4)	13.7 (8)
4th.	125.6 (12)	80.8 (5)	58.8 (7)	23.4 (7)	16.9 (9)
5th.	128.5 (12)	65.5 (9)	36.5 (7)	27.0 (4)	15.7 (8)

	I.—II.	II.—III.	III.—IV.	IV.—V.
RATIOS.	1.66	1.88	1.91	1.75

The class averages given in this table are arithmetical averages. It would have been more correct to follow the indications of the psycho-physic law, and to take the geometrical mean, but as the results obtained by these two methods are very nearly the same, we chose the former to avoid the great labor of calculating the geometrical averages. The numbers in parentheses at the right of the class-averages indicate the number of stars which enter the classes.

Table III. gives the average ratios of all the judgments passed by the nine persons who served as subjects in the first lot of experiments. It involves the classification of 2,120 stars (53 series). The last row of figures represents the final average-ratios of all the results, weighed by the number of series. We shall discuss its significance later.

TABLE III.

CLASSES.	I.—II.	II.—III.	III.—IV.	IV.—V.
Be.—4 s.	1.70	1.94	1.89	1.77
Bo.—12 s.	1.58	1.80	2.18	1.75
Br.—3 s.	1.76	1.96	1.92	1.66
D.—3 s.	1.54	1.97	2.28	1.67
F.—6 s.	1.45	2.21	1.81	1.82
K.—6 s.	1.66	1.90	1.88	1.79
L.—5 s.	1.66	1.88	1.91	1.75
R.—7 s.	1.76	1.87	1.86	1.72
S.—7 s.	1.82	1.99	1.87	1.58
Averages (Weighed).	1.66	1.93	1.96	1.73

At this point the experiments were interrupted by the summer vacation of last year. At the beginning of 1893 Dr. Sanford asked me to complete them.

The falling off of the ratio at both ends of the scale of intensities, for which we could not account (see total average-ratios in Table III.), suggested changing the position of the standards from the extremes (10° and 180°) to a point near the middle of the first and the last classes.

Six series of experiments were taken with the standards at 20° and 160°, and the standards were shown at the beginning and regularly after every five judgments.

TABLE IV.

CLASSES.	I.—II.	II.—III.	III.—VI.	IV.—V.
Leu.—3 s.	1.41	1.76	2.10	1.99
S.—3 s.	1.49	1.45	2.11	1.93
Averages.	1.45	1.61	2.12	1.96

The results (Table IV.) are not based on a sufficient number of experiments to afford a sure basis of comparison with Table III., and in addition to this source of uncertainty, the acquaintance of the two subjects with the scheme of classification had a biasing influence, for in one case, for instance, the subject observed that he was attempting to avoid a mistake which he knew he had made in a previous series, thus judging no more solely from his sensations. The knowledge of the position of the standards in the scale of intensities had also a disturbing effect. The order in which the stars were presented was observed to influence the results. If many stars of about the same intensity were shown successively, the subject would lose sight of the true extent of the scale, and consequently make false judgments. This source of error could not be very great when the standards were shown frequently. The results of these six series do not differ greatly, however, from those of Table III.

The absence of the standards appeared to me the source of a considerable error. During the intervals between their appearance, the subject had in mind a representation of them, unavoidably erroneous in some degree. Even when they were shown regularly at relatively short intervals, as in the last experiments, it often happened that the observer would say, when looking at one of the standards: "I see that I was wrong in my last judgment," thus plainly indicating that he had forgotten the magnitude of the standards. This source of error would naturally bear specially on the last class.

To obviate this defect and to make the conditions of the experiment more nearly those of real star classifications, a new apparatus was made (Fig. 2), in which the standards would

constantly be in sight. The star-making device was also modified in order to allow of the production of a greater number of stars between the limits. A disc (D) seventeen inches

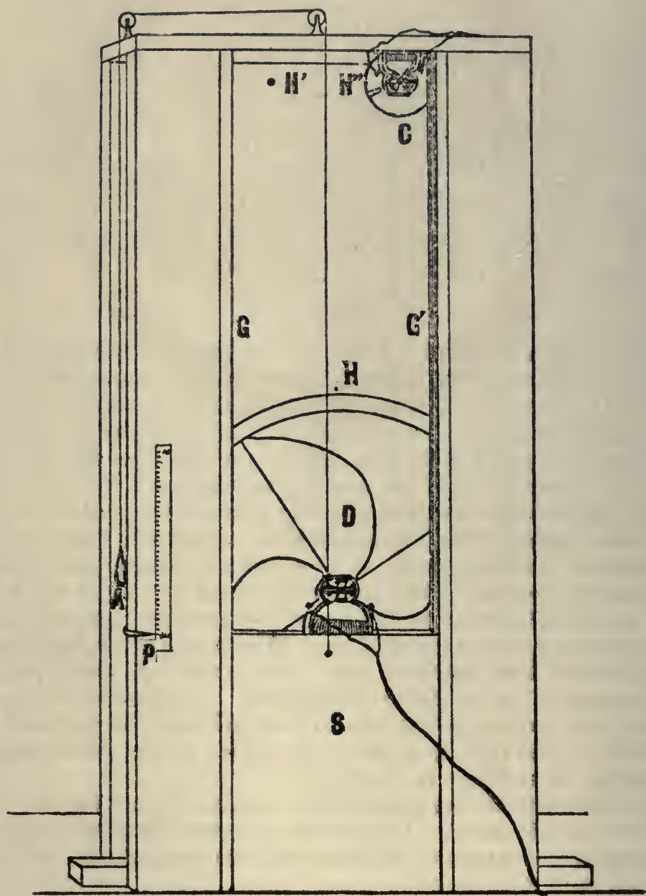

FIG. 2.

in diameter was so cut that as the centre is approached the amount of filled space increases. (See Fig. 3). It is rotated by a small electric motor, fixed on a slide (S). The operator, by means of the handle (A), moves the slide, and with it the disc (D) between the guides (G G′). The position of the disc with reference to the pin-hole (H), through which the

FIG. 3.

light passes, is indicated by the pointer (P), which moves with the slide.[1] The disc can thus be held in fifty different positions, corresponding to the fifty openings expressed in degrees in the figures in the degree columns of Table V. The drawing gives a back view of the apparatus. The subject sat in front, at a distance of about two metres, in a position fixed by a head-rest.

Two other pin-holes (H′) and (H″), the latter hidden by the rotating disc (C), equal in size with (H), give the two standards : (H′), the superior standard, by the free access of the light, and (H″), the inferior, by means of a slit in the disc (C). The light is so placed that it falls equally on the three holes, but before reaching them it passes through a piece of oiled paper, stretched at a little distance from them. Kerosene

[1]It is essential to accuracy that the disc should revolve very close to the hole, especially if the light is not in the same horizontal plane with it. When the disc is at some distance from the hole, and the light comes from above, it may reach the hole through a division of the disc above the one recorded as the measure of the magnitude of the star. A slight error, amounting to a notch of our disc, thus found its way into this second series of experiments. As their purpose, however, was chiefly to show the adaptability of the method to general laboratory use, and as the constant error introduced does not interfere at all with the very general inferences drawn from them, it has seemed best to let them pass.

lamps were used as the steadiest light available. The experiments were conducted in a dark room, the light of the lamps being projected in the direction of the apparatus only.

The instructions to the subject were different from those in the previous experiments. Again he was to classify into five groups at equal intervals from each other, but the standards were now used as limits only, and were not to enter into the classes. When a star appeared to be equal to one of the standards, the subject was to designate it as the Superior or Inferior Limit. He had thus before him a sharply bounded scale of intensities.

This apparatus could also be used for the Method of Equal Intervals. The magnitude of star (H″) can be altered at will by changing the episkotister rotating in front of the hole, as explained in the description of Apparatus I. The subject's task then becomes to find a star at an equal distance in intensity from (H′) and (H″).

Before going further, let me insert here a few observations of some importance in conducting the experiments:

Contrast. In order to avoid contrast effects, the lights used as standards should not be too near the star to be classified. In our apparatus they were at a distance of about fifty cm.

After-Images. The after-images of the standards have no doubt, in some degree, a disturbing influence. Fixating the lights longer than necessary should be avoided. A mere glance, repeated three or four times at short intervals when necessary, was found to yield the best results. In this manner the judgment was often made in memory, when the stars to be classified, as well as the standard, were no more before the eyes. The eyes should be closed between the judgments.

Fatigue varied greatly with the person and with the circumstances. But the irregularities resulting from fatigue were always great enough to render worthless observations taken without regard to it. If comparisons were made in rapid succession for a few minutes, the subject lost confidence in his classifications, and his judgments became clearly erroneous. He would place the same star, seen at a very short interval, in widely different parts of the scale of intensities. For instance, the same star was located now in the second class, and now in the fourth.[1]

A rest of two or three minutes reëstablishes certainty of judgment. When in doubt as to the proper place of a star, the longer one gazes at it continuously, the more confused one becomes. To avoid the effects of fatigue, the stars should

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[1] After-images may play an important rôle in this confusion.

be shown at intervals, varying with the circumstances, and the series of fifty should generally be cut in two, a rest of some half hour being allowed between the two halves.

The results of the first series at least should be rejected. The subject does not realize at the beginning the extent of the scale which he is to divide into five, and, as he advances in the classification of his first series, he very generally perceives that he has made too large or too small a place for one class to the detriment of the others.

The ratios of subject A., in Table VII., decrease with the magnitude, while those of all the other subjects, whose observations are recorded in Table VII., increase with the decrease of the magnitude. The subject stated that knowing the decrease in the relative discriminative power near the limit of visibility, he modified his judgments accordingly, in order to make the five classes differ from each other by the same quantity. This interference with the sensations accounts sufficiently for the inversion of the common order. The ratios without the judgments of this subject are given at the foot of Table VII.

Extension and Color. The stars appeared to be of different extensions, although the holes were of the same size. The brightest star was judged to cover a surface about ten times larger than the one occupied by the faintest. Some of the subjects noticed that their classification was somewhat modified by this extension element. Irradiation, and, perhaps, an illusion of greater surface, produced by a greater intensity of light, may be the cause of this phenomenon.[1] With the kerosene lamps the color of the stars took a yellowish tint as the magnitude increased. This change of color, although very slight, may have influenced the classification.

Let us now take up the group of results obtained with Apparatus II., and compare them with the first group.

Table V. shows the classification of four series, as made by subject Bo.; Table VI. gives the averages of each class in each of these four series, and, at the foot, the ratios. Finally, in Table VII. will be found the class-ratios, based on 1,100 judgments (22 series), made by five persons. At the foot of Table VII. are the general averages weighted, as before.[2]

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[1] A certain complication of conditions is thus introduced, for, the relation of intensity of stimulus to sensation and of the quality of stimulus to the same, are not identical, but in this the classification of artificial stars stands on the same footing as that of real stars.

[2] In two series of subject Be. (Table VII.), who very soon showed the signs of fatigue, the judgments were taken only at the critical points between magnitudes.

TABLE V.

Subject Bo.

Degrees.	1st Series.	2d Series.	3d Series.	4th Series.	Degrees.	1st Series.	2d Series.	3d Series.	4th Series.	Degrees.	1st Series.	2d Series.	3d Series.	4th Series.	Degrees.	1st Series.	2d Series.	3d Series.	4th Series.	Degrees.	1st Series.	2d Series.	3d Series.	4th Series.	Degrees.	1st Series.	2d Series.	3d Series.	4th Series.	Degrees.	1st Series.	2d Series.	3d Series.	4th Series.
360	2	2	1	2	216	2	3	3	3	129	3	3	3	4	77	4	4	4	4	46	5	5	5	5	28	5	6	6	6	16.5	6	6	6	6
334	2	2	2	2	200	2	3	2	3	120	3	4	3	4	72	5	4	4	4	43	5	5	5	5	26	6	6	6	6	15.5	6	6	6	6
311	1	2	2	2	186	3	3	3	3	112	4	4	3	4	67	4	5	4	4	40	5	5	5	5	24	6	6	6	6	14.5	6	6	6	6
289	2	2	2	2	173	3	3	3	3	104	3	4	4	4	62	5	5	4	5	37.5	5	5	5	5	22	6	6	6	6	13.5	6	6	6	7
269	2	2	2	2	161	3	3	3	3	96	4	4	4	4	58	4	5	5	5	35	6	6	5	6	20.5	6	6	6	6	12.5	7	6	6	6
250	2	2	2	2	150	3	4	3	3	89	4	4	4	4	54	5	5	5	5	32.5	5	6	5	5	19	6	6	6	6	11.5	7	7	7	6
232	2	3	2	2	139	3	3	3	4	83	4	4	4	4	50	5	5	5	5	30	5	6	6	6	17.5	6	6	6	6	10.75	7	7	6	7
																														10	7	7	7	7

TABLE VI.

SERIES.	Superior Limit.	I.	II.	III.	IV.	V.	Inferior Limit.
1st.	311 (1)	268.8 (8)	145.5 (8)	83.1 (7)	45. (11)	21.3 (11)	11.2 (4)
2d.		302.2 (6)	179.5 (8)	100.4 (9)	50.9 (9)	21.5 (15)	10.7 (3)
3d.	360 (1)	269.3 (7)	154. (9)	81.3 (8)	44. (9)	19.3 (14)	11.3 (2)
4th.		292.1 (7)	181. (6)	98.9 (11)	47. (9)	20.9 (14)	11.4 (3)
Averages.		283.1	165	90.9	46.7	20.8	11.2
Ratios.			1.72	1.82	1.94	2.25	

TABLE VII.

CLASSES.	I.—II.	II.—III.	III.—IV.	IV.—V.
A.—4 s.	2.06	2.04	2.05	1.73
Be.—4 s.	1.85	1.77	2.01	1.93
Bo.—4 s.	1.72	1.82	1.94	2.25
Leu.—6 s.	1.75	1.82	1.91	2.36
T.—4 s.	1.84	1.81	1.70	2.02
Averages (Weighted).	1.83	1.85	1.92	2.09
Averages Omitting Series of A.				
	1.78	1.81	1.89	2.16

The reader will notice that in the general average the ratio of the average intensity of each class, to that above it, increases as the brightness of the stars decreases, and indications of the same thing appear in some of the individual records of both groups. It is greatest between the classes made up of the faintest stars, and it is smallest between Classes I. and II., where the magnitude is greatest. Since the lower limit of our scale of intensities was a barely perceptible light, and the upper limit star much below the superior light intensity, our results agree well, as far as comparison can be made with those obtained by experimenters who used the Methods of Least Differences.

Prof. Jastrow, in the articles on star magnitude, mentioned above, reaches an opposite conclusion. He finds that "the law regulating the ratio of light between stars of one magnitude and those of the next above it, is the psychophysic law as formulated by Fechner, with the modification, however, that the ratio in question, instead of being perfectly constant, decreases slightly with the brightness of the star."[1]

Other persons (Wolff, Pierce, etc.) obtained similar results by comparing special star-catalogues with photometric measurements.

[1] The extreme ratios are 2.802 between the second and the first, and 1.876 between the seventh and the sixth magnitudes.

A little surprise at this is natural when it is remembered that all the observations not based on star-catalogues show that the relative discriminative sensibility falls when weak or over-strong lights are compared. (See the experiments of Aubert, Masson, Helmholtz, and König and Brodhun.) Moreover, this deviation from Weber's Law is general, and applies also to other sensations than to those of light. Biedermann and Löw, experimenting with weights between 10 and 500 gr., found that the sensitiveness to pressure rose with the increase of weight from ten to 400 gr. and then fell rapidly.

Inasmuch as the scale of intensity began with the seventh magnitude, that is to say, with stars perceptible only to acute sight, it was to be expected that the discriminative powers would be proportionately less in the lower magnitudes, and that, consequently, the ratios between the classes would decrease with the increase of the light intensity.

This constant and well-defined disagreement between the persons who dealt with lights produced and measured for their purpose, and those who started from the star-catalogues, indicates, perhaps, that the early astronomers were influenced in their star classification by some other consideration than the desire to make each magnitude equally different from the next. As the number of the stars is much greater in the lower magnitudes, the lower classes may have been made narrower in range for practical purposes. This is, indeed, a very plausible explanation; what the early astronomers wanted, first of all, was a convenient grouping of the stars, and, although magnitude was, no doubt, taken as the basis of the classification, it seems highly probable that the great difference in the number of stars belonging to the different magnitudes should have modified it, consciously or unconsciously, in the direction indicated by the comparisons considered. When we remember how strong was a similar tendency in some of our subjects, we are inclined to say that it could not have been otherwise.

Indications of a Law of Sense-Memory. If we now compare the average class-ratios of Table III. with those of Table VII., we shall see that the most striking difference is the fall from 1.96 to 1.73 in the ratio at the lower end of the scale, in the results of the experiments made with the first apparatus. This fall finds a ready explanation in the absence of the standards while the comparisons and the judgments were made. There seems to be a natural tendency in us to shift the sensations held in memory towards the middle of the scale of intensities. It might be conceived to operate somewhat as follows. The image of a recent sensation tends to recall, by

association, the united residual of all the past sensations of the same kind, and in so doing passes over, in some degree, to this sub-conscious resultant impression. Something of this kind has long been recognized as a law of the recollection of extreme emotional states, but it has apparently escaped notice as a law of sense-memory also. Something similar was observed by Mr. Bolton in the experiments reported in the first paper of this series (see p. 304); and the experiments of Dr. Nichols also, (AMER. JOUR. PSYCHOL., IV., 1891–92, pp. 75–79), showing, as they do, that practice upon a given rate of tapping tends to draw other rates of tapping toward the practiced rate, give a certain amount of support to the opinion that memories generally tend toward what has been most frequently experienced before.

The amount of modification of the sensations in memory, on account of this tendency, should vary with the time separating the perception of the sensations from the comparison, and with their position in the scale. Starting from a middle range of intensity with a deviation equal to zero, the tendency should increase as the extremes are approached.

Supposing this to be true, we should expect that the lower standard used with Apparatus I., being a just perceptible star, would assume in the subject's memory a magnitude greater than the real. This very plausible hypothesis received confirmation in the second lot of our experiments, in the fact that the subjects generally designated as equal to the lower limit stars superior to it in intensity, although the standard was always in sight; so that the average limit, as determined by the judgments of the subjects, was 11° 38′ instead of 10°, the measure of the real limit. If the error can amount to nearly one-seventh of the stimulus, when comparison is always possible, that is, when the time elapsing between the perception of the standard and that of the object to be compared is very short, it will not be surprising to find that it rises to about one-fifth when the comparison is made with an intensity of light experienced from a few seconds to five minutes, or even more, before the judgment is passed, as in experiments recorded in Table III.

This displacement of the standard in memory would affect chiefly the last class, but also all the others up to that unknown point or region which we called the middle of the scale of intensities.

This change occurring to the images of sensations, if further experiments prove it to be a universal one, must be taken into account in all experiments involving the successive comparison of sensations; for, whatever their nature, what the subject would have in mind at the time of the comparison

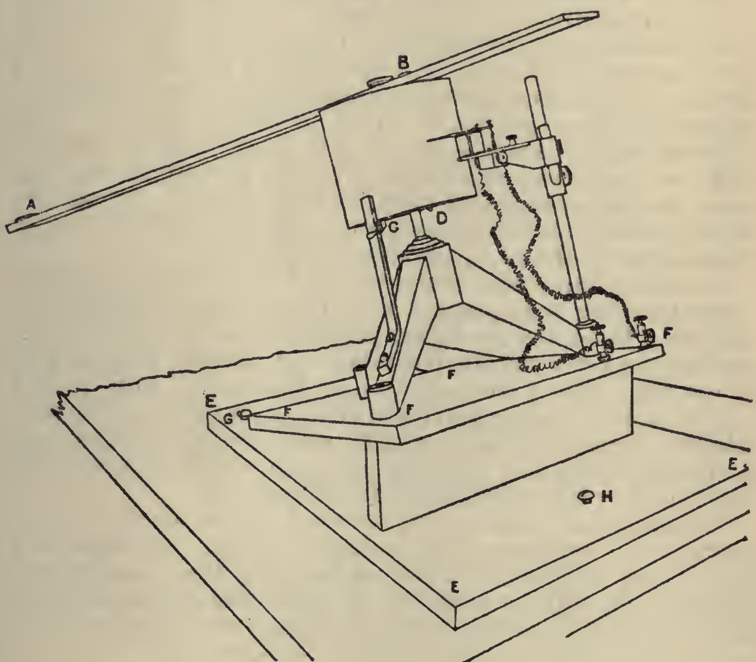
would not be the image of the standard, but another image differing from it in intensity and possibly in form.

Its reality might easily be tested, and, if it proves real, it might be measured by the Method of Right and Wrong Cases (though the experiments would present difficulties), and the construction of the curves representing the displacement in memory of the different species of sensations toward the middle of the scale of intensities, for various lengths of time, would be of considerable value. The influence of this phenomenon on the results of our experiments with Apparatus II. was to decrease the distance, and consequently the ratios, between the classes, as we passed from stronger to weaker stimuli. But as the star-limits were always in sight, the amount of error from this source was comparatively small.

Regarding the results of both groups of experiments from the standpoint of Weber's Law, it is evident that the complete uniformity of ratio required is not shown. The deviations, however, are not extremely great, and the series of magnitudes is very much more nearly a geometrical series than an arithmetical. We feel justified, therefore, in recommending some such apparatus as has been described as a means of introducing star-classification into the laboratory as a psycho-physic experiment.

A NEW PENDULUM CHRONOGRAPH.

By Edmund C. Sanford, Ph. D.



An instrument like that shown in the rude sketch above has been used for two years in the practice work in this laboratory, and as it has proved itself simple and easy to use and tolerably accurate, a description of it may be of interest. In principle the instrument is simply a pendulum swinging completely around in an inclined plane, with a writing surface bent around its axis. This arrangement has two advantages over the ordinary pendulum myograph: the pendulum has no backward swing, and the tracing made by the writing point is a straight line and not a curve.

The instrument was made by fastening a wooden bar upon the top of a "students' drum" of the Cambridge Scientific Instrument Company's pattern, and screwing down the whole on a slightly inclined surface. This bar, lettered *A B* in the cut, has a total length of 1.22 m., a width of 6.4 cm., and a thickness of 1.2 cm. The bar is pierced with a hole 32 cm. from the upper end, which fits snugly about the base of the knob on the top of the drum, and is faced with brass at this point to prevent its wearing loose. The bar is held in place by a thumb-screw (lettered *B*), that passes through into one of the spokes of the drum, and allows the removal of the bar when the drum is to be smoked. A small bob made of two brass plates and weighing 170 grms. is fastened 7.6 cm. from the end of the rod. The drum itself is 13.3 cm. high and 16.1 in diameter, and turns smoothly and easily about a central spindle, a portion of which is visible below it. The inclined base on which it stands is 40.7 cm. long by 38 cm. wide, and makes an angle of 14° with the horizontal base below. In the latter are set three leveling screws, two at the back, one in front, for adjusting it to the horizontal; two of these, lettered *H* and *G*, are shown in the cut.

To secure uniformity in the swings, the pendulum always starts from the same point, *i. e.*, from the catch, lettered *C* in the cut. When the pendulum is ready to fall the peg *D* rests against the catch and the pendulum rod extends upward and to the left. When the pendulum is released, it falls to the left, passes its central point and rises again on the right to a position where it is easily caught with the right hand and carried on again till the peg once more rests against the catch, and everything is ready for another fall.

The time to be measured is inscribed on the smoked paper surface of the drum by a writing magnet. A rack and pinion may be used for raising or lowering this, as shown in the cut, or it may be made to fit tight enough on the post to stay where it is put, as in the instrument in this laboratory.[1]

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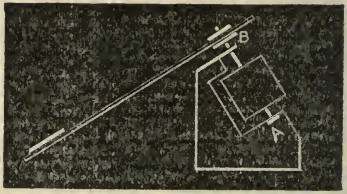
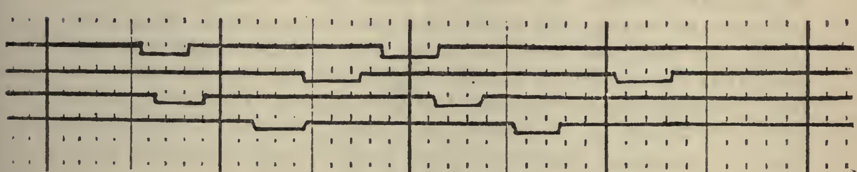
[1]A few tests were made at the start with a still simpler apparatus. This was nothing more than a wooden cylinder, about three inches in diameter, with a steel rod running through it, with a point at the bottom and a collar and nut at the top for attaching the pendulum bar, something as shown in the cut in the margin. The point of the rod rested in a little hollow in a bit of brass at *A*, and was supported by a Y of brass at *B*. The results showed a considerable degree of acuracy.



In order that such an instrument should give accurate results, two things must be regarded: the successive swings of the pendulum must be made in equal times, and the inertia of the writing magnet must be known or compensated. In a recent test of twenty-four successive swings of the pendulum, made just after oiling the spindle, there was a gradual change in the time required for the pendulum to traverse a marked space, corresponding to a little more than a second of time in the middle of its swing, of about .002 sec. Other tests, however, gave less favorable results. In 190 swings, taken ten days later without re-oiling, each tenth swing being timed, the extreme difference reached .009 sec.; but in a third set of 13 consecutive swings, taken on another occasion after re-oiling, it fell again to .003 sec. The difference from set to set, due perhaps to differences in leveling or lubrication, was greater than these, amounting to .018 sec. between the fastest swing measured and the slowest. An error from this cause might perhaps reach one part in fifty between single selected swings, but with care could probably be kept much smaller, and in an average at all events would certainly be so. This error is also distributed over a considerable portion of the swing, and is smaller absolutely when shorter intervals are measured.

The error of the magnet could be measured upon the drum itself by arranging for the making and breaking of an electric circuit at a fixed point by the motion of the drum, but it is simpler to exclude the error by using the same movement of the armature of a single magnet to indicate both the beginning and end of the time to be measured, as indicated in the cut below. This is accomplished without difficulty by put-



ting the beginning and ending keys in parallel circuit and letting the closure of the first be shorter than the time to be measured. In case the presence of a constant error is unimportant, as, for example, in reaction-times, where relative results are aimed at, the use of the magnet may still further be simplified by making the closure of the circuit correspond to the beginning of the time to be measured and its opening

with the end. The error introduced is the difference between the inertia of the magnet at the make and its inertia at the break. If a Deprez signal is used as a time-marker, this difference should not exceed .003 sec. and with careful adjustment would be practically zero.

A defect of such an instrument is that it does not move evenly throughout a swing, but first slowly, then rapidly, and at last slowly again, so that a given distance on the surface of the drum does not everywhere mean the same amount of time. The simplest way to obviate this is to accompany the time tracing with a parallel tuning-fork tracing, but this requires a laborious counting of the waves in the latter. An easier way, when the successive swings are tolerably uniform, as in this instrument, is to take one tuning-fork tracing, make it permanent, and stick it, taking care not to stretch the paper, on the edge of ruler. When this has once been counted and marked off, it can be applied to any time tracing and the amount read off, at once. A still more expeditious way (used already by Dr. Bowditch upon pendulums) is to have the scale printed directly on the paper with which the drum is covered. If the drum is lightly smoked the lines of the scale can be seen through the blackening and the time tracing read at once in units of the scale, and by estimate to a tenth of one of them.[1] In the cut above a portion of such a scale is shown; the finest divisions give hundredths of a second directly, and thousandths by estimate. An intelligent engraver can cut a block for printing a scale of this kind from a tuning-fork tracing taken with a Deprez signal or other time-marker; but he should be cautioned with regard to accuracy, and especially to get the scale lines exactly parallel, otherwise another error is introduced. An error is also apt to creep in in the estimate of tenths, but should not amount to more than one-tenth, so that the total error of any single measurement, provided the scale is exact, ought not to exceed three or four thousandths of a second for short times, and one part in fifty for times of a second or over, a degree of accuracy that is sufficient for all practice work in psychological time measurements and for many kinds of research.

This kind of a chronograph lends itself easily to measurements of any time intervals less than about two and a half seconds; the time of a whole swing is nearly four seconds, but the scale is so crowded at the ends that they are practically useless. Anywhere in the middle second of the swing

[1] When the record has been read the paper need not be removed from the drum, but may be wiped off and resmoked, and this may be repeated a number of times.

the scale is open enough to admit of estimating tenths of a division. All the ordinary reaction- and association-time experiments fall below this limit, and the instrument is useful for that sort of experiment because it is nearly noiseless.

The chronograph can readily be adapted for other time experiments. If two break circuit keys are arranged so that they may be opened by the turning of the drum, Exner's interesting experiments on nearly simultaneous sensations in different senses can be repeated, and the drum itself can be used to measure the time between the stimuli, while at a few minutes' notice these fittings and those for the chronograph can be removed and the drum restored to its original purposes.

If the laboratory already has a drum, writing magnet and electrical tuning-fork, as most with full equipment have, the expense of setting up the chronograph (aside from the block for printing scales, which should cost from 10 to 15 dollars) is not great; if these have to be bought new the instrument will cost between 40 and 50 dollars.

LABORATORY COURSE IN PHYSIOLOGICAL PSYCHOLOGY.

By Edmund C. Sanford, Ph. D.

(*Fourth Paper.*)

V.—VISION. (*Continued.*)

Seeing of Light and Color.

The aim of the following experiments is not to adjudicate conflicting color theories, but rather to present the most important experimental facts that all color theories must regard.[1] Authoritative accounts of the theories may be found as follows: Young-Helmholtz theory; Helmholtz, Handbuch der physiologischen Optik,[2] 2te Aufl., pp. 344-350. G[1] 290-294, 320-321, 367; F. 380-387, 424-425, 484. Also Popular Scientific Lectures, 1st Series, New York, 1885, pp. 249-256. Hering's theory; Hering, Zur Lehre vom Lichtsinne, pp. 70–141, (two communications to the Vienna Academy, April 23 and May 15, 1874); Ueber Newton's Gesetz der Farbenmischung, pp. 76-79, a very brief account of his own in connection with a general account of theories. These are the most prominent theories, and something on them, especially on the first, will be found in the physiologies and in some works on the use of color in the arts. Other theories more or less different from these will be found as follows: V. Kries: Die Gesichtsempfindungen und ihre Analyse, Du Bois-Reymond's Archiv, 1882, Supplement-Band, vi, 1-178. Wundt: Physiol. Psychol., 2te Aufl., pp. 453-456; 3te Aufl., 491–496. Also Philos. Studien, IV, 1888, 355-389. Donders: Ueber Farbensysteme, Archiv für Ophthalmologie, XXVII, 1881, H. 1. Noch einmal die Farbensystem, *ibid.*, XXX, 1884, 1. Göller: Die Analyse der Lichtwellen durch das Auge. Du Bois-Reymond's Archiv, 1888. Christine Ladd Franklin, Eine neue Theorie der Lichtempfindungen. Zeit. für Psychol., IV, 1892, 212.

On color vision in general may be mentioned, besides these works of Helmholtz, Hering and Wundt: Fick: Qualität der Lichtempfindungen, Hermann's Handbuch der Physiologie, III, Th. i, pp. 160-232. Maxwell: On the Theory of Compound Colours

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[1]For concise statements of these facts, see Wundt, Physiologische Psychologie, I, 487 (cited by Ladd, Phys. Psych., 338), also p. 501, and Christine Ladd Franklin, Zeit. für Psych., IV, 1892, 212.

[2]The second edition of Helmholtz's great work is as yet incomplete. The latest complete edition is the French translation, Optique physiologique, Paris, 1867. To facilitate reference when pages are cited, the numbers are given preceded by G[2] for the second German edition, and by G[1] for the first German edition, and by F. for the French translation. Occasional errors in the pages for G[1] may have crept in, for that edition was not at hand and the pages for it have been taken from the double paging in G[2] and F. The error can hardly amount, however, to more than a page one way or the other.

and the Relation of the Colours of the Spectrum, Phil. Trans. 1860; and On Colour Vision, Proc. Royal Institution of Great Britain, VI; reprinted in Maxwell's Scientific Papers, I, 410-440; II, 267-280. Rood: Students' Text-book of Color, New York, 1881. Aubert: Physiologie der Netzhaut, Breslau, 1865; also Grundzüge der physiologischen Optik, Leipzig, 1876, pp. 479-572. This work forms part of the second volume of Graefe und Saemisch's Handbuch der gesammten Augenheilkunde. Charpentier: La Lumière et les Couleurs, Paris, 1888. Von Bezold: The Theory of Color in its Relation to Art and Art Industry, Boston, 1876. Benson: Manual of the Science of Colour, London, 1871, pp. xii, 58. Chevreul: The Principles of Harmony and Contrast of Colors, London, 1859. Le Conte: Sight, New York, 1881. Ladd: Elements of Physiological Psychology, New York, 1887. Beaunis: Nouveaux Éléments de Physiologie Humaine, Paris, 1888; and other standard physiologies.

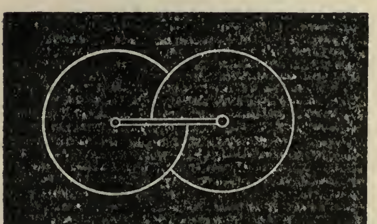
Apparatus. In addition to the blue and red glass, the colored papers and the black and white cardboard used in the previous section, there will be required for this, pieces of yellow, green and violet glass, or of colored gelatine (see below), a small pane of clear glass, a mirror, a sheet of white tissue paper or other semi-transparent paper, and pieces of gray paper or cardboard of different intensities. Gray papers can be made by painting white paper over with India ink; or a tolerable substitute may be made by overlaying black paper or cardboard with one or more thicknesses of tissue paper.

For some of the contrast experiments (Ex. 141 ff.) the gummed parquetry rings and the lentil dots used by the kindergarteners are extremely convenient, and are not expensive. The rings are 1, 1 1-2 and 2 in. in diameter and 1-8 in. broad and are to be had in a considerable variety of colors; the dots are 1-4 in. in diameter, and in six colors. See catalogue of the Milton Bradley Co., Springfield, Mass., pp. 49 and 71.

The standard of color when exactness is important is, of course, the spectrum; and experiments with pure (*i.e.*, monochromatic) spectral colors are the final appeal. The apparatus required for a complete study of color sensations with spectral colors, especially when quantitative results are aimed at, is extremely refined and correspondingly expensive. The spectrophotometer pictured on p. 355 of Helmholtz's Physiologische Optik, 2te Aufl., is quoted by the makers, Franz Schmidt und Haensch, Stallschreiber-Strasse 4, Berlin, S., at mk. 750. Other apparatus of similar purpose ranges from mk. 375 to mk. 3500. The spectrophotometer of the Cambridge Scientific Instrument Co. costs £15. Simple qualitative experiments like those of this section can, however, be made without very expensive apparatus, and for the most part without spectral colors. Where spectral colors are employed a simple prism (costing from 15 cents upward), or at most an ordinary spectroscope, such as is found in every chemical and physical laboratory, will serve amply. A pocket spectroscope even, costing from $5 or $6 upward, will show a good deal, and is useful in determining approximately the composition of light transmitted by colored glass. If nothing more is done, it is desirable that the experimenters familiarize themselves with the appearance of the spectral colors and the chief Fraunhofer lines as landmarks in the spectrum. By combinations of thin sheets of colored gelatine, light that is practically monochromatic can be secured; see a paper by Kirschmann, Ueber die Herstellung monochromatischen Lichtes, Wundt's Philos. Studien, VI, 1891, pp. 543-551. Such sheets are used before calcium lights in the

theatre for the projection of colored lights upon the stage, and may be had of dealers in stereopticons. A. T. Thompson & Co., 13 Tremont Row, Boston, sell red, yellow, green, blue, violet and purple in sheets, 20 1-2x16 3-4 inches, at 25 cents a sheet. For many purposes these sheets are as good or better than colored glass.

For the study of the phenomena of color-mixing with artificial colors, the most satisfactory instrument is the color top or rotation color-mixer in some one or other of its numerous forms. One of these was mentioned in the introduction to the previous section on the Mechanism of the Eye and Vision in General, namely, a little electric motor. All the experiments of this section that require a color-mixer can be made with such a one. Many if not all of them could be made with the color tops sold as toys, or with the very simple one suggested by Dr. Bowditch in his Hints on Teaching Physiology, to wit, a button-mold fitted with a peg and spun with the fingers. One made of a button-mold an inch and three quarters in diameter and carrying disks two and a half inches in diameter, shows the contrast effects of Ex. 142*d* as elegantly as could be desired. The disks are held in place by a piece of rubber tubing of very small bore fitting snugly upon the stem and twisted down upon the disk like a nut. Larger apparatus specially designed for color-mixing may be had of all physical instrument dealers. Among the rest may be mentioned the *Farbenkreisel* made by R. Rothe, Mechaniker des physiologischen Instituts der k. k. Universität, Prag (Wenzelsbad), at mk. 30. A fine instrument by the same maker for rotating a horizontal disk either by foot or hand, with additional parts for studying the color-blindness of the peripheral parts of the eye, costs mk. 160. The color-mixer of the Milton Bradley Co., Springfield, Mass., costs $10, including disks, etc.; the color-mixers of the Cambridge Scientific Instrument Co., St. Tibb's Row, Cambridge, England, cost £6-10 and £10. R. Jung, Heidelberg, has rotation apparatus, including one that moves by clock-work at mk. 50-65 with disks. The important thing in such a piece of apparatus is that it should rotate rapidly enough to give a smooth and steady mixture of two colors when these occur but once each upon the disk, *e. g.*, to give an even gray with a disk that has a solid 180° of black and a solid 180° of white. When this is the case the two disks may be slipped together, as in the cut, and any required proportion of the colors easily arranged. If the rotation is not sufficiently rapid the sectors must be made smaller and more numerous, *e. g.*, four sectors of black of 45° each separated by four sectors of white of the same size. This is not difficult when the proportions of color are to remain constant, but where adjustments are to be made the multiplicity of sectors is a disadvantage. Rothe and the Milton Bradley Co. supply colored paper disks in considerable variety evenly cut, and this is an important point, for if the cutting is inexact the disks will appear with bothersome fringes of color when in rotation. The centre hole in Rothe's disks is of course cut to fit the Rothe color mixers. His disks may be had in two sizes, 20 cm. and 11 cm. in diameter, at prices ranging from 60 to 105 kr. per doz. for the large, and 20 to 30 kr. for the small, according to color. Colored papers of excellent color and surface (shiny papers are to be avoided) may be had of R. Jung, Heidelberg.



A rotation apparatus that will serve excellently for class demonstrations is a carpenter's polishing lathe, which is to be obtained at some hardware stores and sells at $4.50. It can be screwed to the table and worked by the hand or foot. Unfortunately it can hardly be made to rotate rapidly enough to blend 180° of white with 180° of black, but for fixed disks with more numerous sectors it works excellently; and though made for so remote a purpose can be used without change and will carry disks up to a foot in diameter. With a very little alteration it would carry them twice as large. Perhaps a maximum of simplicity is reached in the use of a boys' "buzzer" as a color-mixer, which the writer has heard recommended, but never tried. Special disks for use in certain experiments are shown in cuts accompanying them.

In addition to the color-mixer and disks, a stereoscope and stereoscopic diagrams (see cuts accompanying the experiments) and a double refracting prism will be needed. Any stereoscope will answer, but the hood and the central partition should be removed. The double refracting prism may be purchased at no very great expense from dealers in physical instruments.

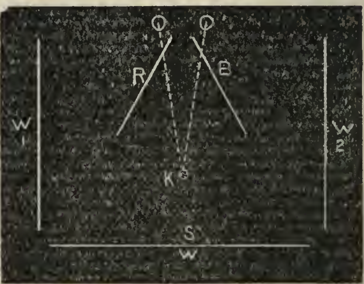
In Ex. 142*b* and 148*b*, a small wooden frame made by fixing two pieces of board 6x6 in. together at right angles, is needed (see diagram accompanying Ex. 142*b*). The convenience of the instrument is much increased if guide strips of wood or metal are attached to the vertical and horizontal pieces, so that the diagrams to be used upon them will be held in place and yet be easily interchangeable. For exhibiting a very deep black in Ex. 130*a*, a black box may be prepared as follows. Make a light tight wooden box 8x10x12 inches in size; cut a two-inch hole in one end and have the whole painted a dull black, both within and without. Before closing it finally, divide it by a slanting partition running obliquely upward and forward from the back edge of the bottom to a point on the top about four inches from the front. The front side of this partition should be covered with black velvet. In comparison with the black that is seen on looking through the hole against this slanting velvet, the gray character of the black paint, of ordinary black cardboard, and even of black velvet, is easily recognized.

The easiest test for color-blindness is made with colored worsteds, which may be had of any dealer in oculists' supplies. An approved selection of colors in sufficient variety is sold by N. D. Whitney & Co., 129 Tremont street, Boston, Mass., at $2.50. A small card of wools for testing color vision is to be found on the inside cover of Galton's Life History Album, London, Macmillan & Co., 1884.

Apparatus is helpful in measuring out the color fields in Ex. 128, though it need not be elaborate. At its simplest two things are necessary: something for steadying the head, and a broad surface perpendicular to the line of sight on which to map out the color fields. A block on which to rest the chin and a convenient wall might do. If something a little more permanent is desired, the headrest shown in the last section (*American Journal of Psychology*, IV. 1891-92, 474) can be clamped to the front edge of a narrow table, and a screen of light boards (or better, a wooden frame covered with black or gray cloth) fastened about a foot back from it. This distance must not be great, or the screen will need to be of excessive size. It is well to paste a vertical and horizontal scale upon the screen crossing at the point immediately before the eye, so that distances may at once be read off. Such an instrument is known as a *campimeter*. A more perfect instrument for this purpose is the *perimeter*. Of this instrument there are many forms; that of Priestly Smith is perhaps as convenient as any except the most elaborate

ones. In this instrument, not to attempt a detailed description, the place of the screen is supplied by a curved arm that can be turned about an axis at the point on which the eye is fixed, and in turning would describe a hemisphere of which the eye is the centre and the fixation point the pole. The arm is marked for every 5°, and the limits of the field of vision on any meridian can at once be read off and recorded. The record is made by a needle prick in a diagram carried by the instrument, a new diagram being inserted for each eye tested. The price of this instrument from R. Jung, Heidelberg, is mk. 60, from New York dealers $30.

For Ex. 145 and 151 an apparatus devised by Hering and described by him in the *Zeitschrift für Psychologie*, I, 1890, 23-28, is extremely convenient. It is made by R. Rothe of Prag at 28 marks. The apparatus is simple, however, and any carpenter can make of wood one that will answer. The first aim of this instrument is to secure a binocular mixture of blue and red. For that purpose blue and red glasses before the eyes may be used, provided that a good deal of white can also be mixed in with the color of the glass. This is done by letting the glasses stand at an angle before the eyes and reflect on their upper surface the images of suitably placed white screens. The quantity of white light is regulated by the position of the screens with reference to the source of illumination and by the inclination of the colored glasses. The following cut shows diagrammatically what the arrangement of glasses and screens is.



In the cut W_1 and W_2 are the screens just spoken of, *R* and *B* the red and blue glasses, *W* a white surface carrying a narrow black strip at *s*, and *k* is the point upon which the eyes are fixed. In an instrument made by a carpenter for the laboratory of Clark University, the following plan was followed; it is here reproduced not because it is the best, but for the sake of definiteness. The stuff used in the instrument was almost all seven-eighths of an inch thick, and that thickness may be understood except where something else is stated. The base is a board 30 in. long, 12 in. wide. In the middle of this is placed another board 12 in. long and 10 in. wide, leaving a margin of an inch on each side and of nine inches at the ends. This little platform bears the white cardboard corresponding to *W* in the diagram. On the nearer edge of this platform is fastened an upright post 15 in. high, 3 in. wide. At its upper end on the forward side this post carries the frames that hold the glasses *R* and *B*. The glasses are 4 in. square, and are framed on three sides only, the upper edge being left free so that the glasses may come close to the eyes. The frames are small pieces of board 6 in. long, 5 in. wide with a square piece (three and three-quarters inches on the side) taken from the middle of their upper ends, leaving them like a U with very square corners and a heavy bottom. Over these square holes the glasses are fastened. The frames are fastened with a single screw each to the post, the screws penetrating the frames about an inch and a half from the free edge of the glass. When in position, the glasses rise about three-quarters of an inch above the top of the post, and stand like the sides of a roof. They do not quite meet, however, but leave a space for the observer's nose between them when the apparatus is in use. The screws that hold

the frames should be tight enough to hold the frames in position, but not so tight as to prevent their turning in adjustment. On the front of the post and about six inches upward from its foot is a wire about three and a half inches long, extending forward from the surface of the post and perpendicular to it. At its end is a little button of cork, the fixation point *k* in the diagram. The side screens of the instrument are exactly alike and the description of one will do for both. Each screen is a piece of half-inch board 9 in. wide and 13½ in. long. This board turns midway from top to bottom on a horizontal axis and in a light frame just large enough to enclose it. The frame itself is fastened to a broad piece of board which forms its base and rests in turn on the base board of the instrument. A peg in the middle of the first of these, fitting into a hole in the last, allows the rotation of the frame and screen about a vertical axis. The screen is thus made adjustable in any direction. Its face is covered with white cardboard. The only remaining part of the instrument is the strip of black paper, a quarter of an inch wide, represented by *s* in the diagram, which is pasted on *W* perpendicular to the post. It is highly important that *W* be without speck or spot, and that the colored glasses be as free from flaws as possible. The instrument as described is intended for binocular contrast. For binocular color-mixing, other pieces of glass besides red and blue are needed for other combinations and the black strip is not required.

Another simple demonstrational instrument of Hering's contriving is for the study of changes of brightness in colors and can also be adapted for contrast. Its principle is the same as that used in the side screens in the last instrument, namely, change of position with reference to the source of illumination. A white card, provided that its surface is not shiny, receives a maximum of light and looks brightest when it stands perpendicular to the light. As it is turned and the light falls obliquely upon it, it receives less and less and looks darker and darker. If it is shiny, as most paper and cardboard are, this change is not uniform, but this does not much interfere in this instance. The paper used should, however, be dull finished. The instrument at its simplest is a tall box open in front and with a hole in the top to look through. It is painted black inside and contains a screen that can be turned about a horizontal axis, and thus receive light perpendicularly or obliquely as desired. It is, however, convenient to have a frame instead of a permanent screen so that a number of cards of different color or brightness can be interchanged, and to have the box double so that two frames can be used side by side and comparative tests can be made. When contrast is to be introduced, a second pair of frames above the first and high enough so as not to shade them are introduced. The cards that are used in these upper frames must each be pierced with a hole, say 2x4 cm., near the middle, in such a position that when the eye looks through them from the top of the box, nothing but the card in the frame below can be seen. The hole must be carefully and cleanly cut, and the edge, if it shows white, must be colored like the surface of the card. When such a hole is looked at with a single eye, it is easy to conceive the part of the lower card seen, not to be really below, but a part of the upper card itself. This illusion might be strengthened by the use of a feebly convex lens to exclude exact accommodation. Changes of the inclination of the upper frame (provided there is no reflection from its under surfaces) can produce no real change in the illumination of the lower one, but very striking changes seem to follow such changes of the upper one. This instrument in finished form, though without the additional

frames for contrast, can be had like Hering's other apparatus of his mechanic, R. Rothe, Prag.

Many of the color experiments to follow can be demonstrated before a considerable audience by the use of a projection lantern, and some makers of lanterns have diagrams for contrast colors, etc. Their preparation, however, can offer little difficulty to those familiar with the use of the lantern and with the ordinary forms of the experiments.

A given color sensation may be changed in three ways: in *color-tone*, in *intensity*, and in *saturation*, or to use Maxwell's terms, in *hue*, *shade* and *tint*. Changes in *color-tone* are such as are experienced when the eye runs through the successive colors of the spectrum. Changes in *intensity* are changes in the brightness of the color. Changes in *saturation* are such as are produced by the addition of white; when much white light is added, the color is a little saturated. Changes in intensity and saturation if excessive involve some change of color-tone also. The number of primary colors is various in various theories; red, green and violet (or blue) are selected by the supporters of the Young-Helmholtz theory, red, green, yellow and blue by Hering, Mach and others, while Wundt is indisposed to make any particular ones more original for sensation than the rest.

127. Color-blindness. Holmgren's method. Spread the worsteds on a white cloth in good daylight. Pick out a light green (*i. e.*, a little saturated green) that leans neither toward the blue nor the yellow; lay it by itself and require the person to be tested to pick out and lay beside it all other skeins that are colored like it, not confining himself, however, to exact matches, but taking somewhat darker and lighter shades also, so long as the difference is only in brightness and not in color-tone. Do not tell him to pick out "the greens" nor require him to use or understand color words in any way; simply require the sorting. If he makes errors, putting grays, light browns, salmons or straws[1] with the green, he is color-blind; if he hesitates over the erroneous colors and has considerable difficulty, his color-vision is probably defective, but in a less degree. If the experimentee makes errors, try him further to discover whether he is red-blind or green-blind by asking him to select the colors, including darker and lighter shades, that resemble a purple (near magenta) skein. If he is red-blind, he will err by selecting blues or violets, or both; if he is green-blind, he will select green or gray, or both, or if he chooses any blues and violets, they will be the brightest shades. If he makes no errors in this case after having made them in the previous case, his color-blindness is incomplete. Violet blindness is rare. Complete certainty in the use of even such a simple method as this is not to be expected without a full study of the method and experience in its application.

On color-blindness and methods of testing for it cf. Helmholtz: Op. cit. G² 357 372, 456-462; G¹ 294-300, 847-848; F. 388-400. Jeffries: Color-blindness, its dangers and its detection, Boston, 1879 (this work contains a seventeen-page bibliography on color-blindness and kindred topics); also an article on Color-blindness in the Reference Handbook of the Medical Sciences, New York, 1886, II, 241. Rayleigh and others: Report of the Royal Society's Committee on Colour Vision. Proc. Royal Soc., LI, No. 311, July 19, 1892. Hering: Zur Diagnostik des Farbenblindheit, Archiv für Ophthalmologie, XXXVI, 1890, Heft i, 217-233; also Die Untersuchung einseitiger Störungen des Farbensinnes mittels binocularer Farbengleichungen. Arch. f. Ophthal., XXXVI, 1890, H. 3, 1-23. See also a

[1] It is difficult to give the tints accurately in words. The experimenter should consult the colored charts given in the books of Jeffries mentioned in the bibliography.

paper by Hess in the same place, pp. 24-36. Kirschmann: Beiträge zur Kenntniss der Farbenblindheit, Wundt's Philos. Studien, VIII, 1892, Hefte 2 u. 3. Helmholtz, Hering, Kirschmann and others give exact methods for determining the particular colors that are lacking. On differences in the apparent extent of the spectrum in different observers, see Morgan: Animal Life and Intelligence, pp. 280-283.

128. Vision with the peripheral portions of the retina. *a.* Campimetry. Color-blindness is normal on the peripheral portion of the retina. At the very centre the pigment of the yellow spot itself interferes somewhat with the correct perception of mixed colors containing blue (cf. Ex. 110). In a zone immediately surrounding this all colors can be recognized. Outside of this again is a second zone in which blue and yellow alone can be distinguished, and at the outermost parts not even these, all colors appearing black, white or gray. The zones are of course not sharply bounded, but blend into one another, their limits depending on the intensity and area of the colors used. *a.* With the campimetrical apparatus at hand, find at what angles from the centre of vision on the vertical and horizontal meridians of the eye the four principal colors, red, yellow, green and blue, can be recognized; try also for white. Keep the eye steadily fixed on the fixation mark of the instrument and have an assistant slide the color (say a bit of colored paper 5 mm. square pasted near the end of a strip of black cardboard an inch wide) slowly into the field from the outside. It will be well to move the paper slowly to and fro at right angles to the meridian on which the test is made, so as to avoid retinal fatigue. Take a record of the point at which the color can first be given with certainty. Repeat several times and average the results. The size of the colored spot shown should be constant for the different colors, and the background (preferably black) against which the colors are seen should remain the same in all the experiments. *b.* Repeat the tests with a colored square 10 mm. on a side, and notice the earlier recognition of its color as it approaches from the periphery. *c.* Try bringing slowly into the field (best from the nasal side) bits of paper of various color, especially violet, purple, orange, greenish yellow and greenish blue; or better, hold the bit of paper somewhat on the nasal side of the field and turn the eye slowly toward it, beginning at a considerable angle from it. If the paper is held before a background, containing a line along which the eye can approach the paper, the eye will be assisted in making its approach gradual. Observe that on the outer parts of the retina these colors first get their yellow or blue constituents, and only later the red or green, and appear in their true color. If the range of choice is sufficiently large, it may be possible to find a red (inclined toward red purple), a green (inclined toward the blue), which, like pure blue and yellow, change only in saturation and not at all in color tone as they move inward toward the centre of the field. These four colors are the *Urfarben* or primary colors of Hering.

Helmholtz: *Op. cit.*, G2 372-374, F. 399-400. Hess: Ueber den Farbensinn bei indirektem Sehen, Archiv für Ophthalmologie, XXXV, 1889, H. 4. Hering: Ueber die Hypothesen zur Erklärung der peripheren Farbenblindheit, Arch. f. Ophth., XXXV, 1889, H. 4, pp. 63-83; XXXVI, H. 1, 264. Fick: Zur Theorie des Farbensinnes bei indirektem Sehen, Pflüger's Archiv, XLVII, 1890, 274-285. Aubert, Phys. Opt., 539-546.

129. Changes in color tone. With spectral lights, change of vibration rate, if not too small, means change of color tone, but equal changes in vibration rate do not involve equal changes in color tone. The change of color tone is most rapid in the green region of the spectrum, less rapid at the red and violet ends. *a.* With the spectroscope and daylight find the characteristic *D*, *E*, *F* and *H* lines. The *D* line lies in the golden yellow, *E* in the green, *F* in the blue, and *H* at the end of the violet. Between the *D* line and

the F line, the vibration rate changes from 526 to 640 billion vibrations per second, and the color runs from yellow, through green to blue, while from F to H, with the greater change in vibration rate from 640 to 790 billion per second, the change is only from blue to violet. *c.* Notice the tendency of the succession of spectral colors to return upon itself, shown in the resemblance of the red and violet.

Helmholtz: *Op. cit.*, G[2] 289, 320, G[1] 237, F. 319. Rood: *Op. cit.*, 27. Wundt: *Op. cit.*, I, 429. Fick: *Op. cit.*, 173-175, 183. Aubert, Phys. Opt., 530.

130. Changes in intensity. Black and white. Black and white are the extremes of intensity in the series of grays. The ordinary black and white of conversation are considerably short of these extremes. *a.* Compare a bit of black velvet or black cardboard with the black of the black box described above. *b.* Compare ordinary white paper in diffused light with the same in direct sunlight or with a brightly illuminated white cloud. *c.* Just observable differences with medium intensities. Prepare a disk like that shown in the accompanying cut by drawing along a radius of the disk a succession of short lines of equal breadth. Let the breadth of the line correspond to about one degree on the edge of the disk. Since the breadth of the line is everywhere the same, it will occupy a relatively greater portion as it nears the centre. When the disk is set in rapid rotation, each short line will give a faint gray ring, those at the outer edge being very faint, those nearer the centre, darker. Find which is the faintest ring that can be seen, and calculate the proportions of black and white in it.[1] The ratio of the black to the white measures approximately the just observable decrease in intensity below the general brightness of the disk. The results of Helmholtz and Aubert are respectively: Helmholtz, 1:117 to 1:167. Aubert, 1:102 to 1:186, the differences depending on the intensity of the general illumination of the disk. Some wandering of the eyes is helpful, but too rapid motions of the eyes, which tend to break up the even gray of the rings, must be avoided. It is absolutely essential to have the rotation very rapid and perfectly free from vibration — so rapid that with the moderate motions of the eyes, the uniform gray of the rings is not disturbed. If great rapidity is impossible, replace the single black line by two of proportionately less breadth on opposite sides of the disk, or by four at 90°.

Helmholtz: *Op. cit.*, G[2], 384-393, G[1], 310-316, F. 411-419. Aubert: Physiol. Optik., 489-492.

131. Changes in intensity. Colors other than black and white. At their maximum intensity, all colors tend toward white or yellowish white, green becoming first yellow and then white, red progressing hardly beyond the yellow, but blue and violet easily reaching white.

[1] The formula for the amount of black, assuming that the radial line is absolutely black, and taking some arbitrary point of the line, *e. g.*, the middle point, for the calculation, is of course $\frac{b}{2\pi r}$, where b is the breadth of the radial line and r the distance of the chosen point from the centre of the disk.

a. Fix a prism in the sunlight so that it projects an extended spectrum on the wall. Hold a card, pierced with a pin-hole, before the eye, and bring the eye successively into the different colors, looking meanwhile through the pin-hole at the prism. Something of the same kind may be seen by looking through pieces of colored glass at the disk of the sun behind a cloud (in which case the portions of the cloud seen at the sides of the glass afford a means of comparison), or at the image of the sun reflected from an unsilvered glass plate, or by concentrating light from colored glass on white paper with a convex lens. *b.* It is easy to reduce the intensity of colors with the color-mixer by spreading the light of a colored sector over the whole surface of a disk otherwise black.[1] Make a succession of mixtures of red and black on the color-mixer, beginning with a proportion of red that makes a barely observable change, and increase the proportion till the red decidedly predominates in the mixture. Place a smaller disk of black over the larger disks so as to have a standard black in the field. If any of the red shows through either black disk several of the latter should be used together to prevent it. Try also with the other chief colors. Disks like the diagram (in which shaded parts stand for color and solid black for black) show the whole series of such gradations at once, though not quite so satisfactorily. *c.* Carry a number of small slips of colored paper into a darkened room, or look at them through a fine needle hole in a card, and notice the order in which they lose their color quality. *d.* Adjust the spectroscope so that the chief Fraunhofer lines can be seen, and then gradually narrow the slit through which the light enters the instrument. Observe that red, green and violet-blue with a trace of yellow persist longer than the intermediate colors, and that when all the color is gone, there still remains some light in the region of the green. This experiment must be performed in a dark room, or the observer must envelope his head and the ocular of the instrument with opaque cloth. *e.* Purkinje's phenomenon. In a light of moderate brightness, choose a bit of red paper and a bit of blue paper that are about equal in intensity and saturation, or better, make such a pair with the color-mixer by adding white or black till the intensity and saturation appear the same. Carry both into the full sunlight and notice which appears brightest. Carry both into a darkened room, or observe them in deep twilight, or through a very fine needle hole in a card, or even with nearly closed eyes, and again notice which seems brightest. Cf. also Ex. 142*a*.

Helmholtz: *Op. cit.*, G² 402-444; on *a*, 284-285, 322-324, 465, 466; on *b*, 469, 471-472; on *e*, 428-430, 443-444. G¹, 234, 280, 281, 317-321. F. 315, 369, 370, 420-425. Fick: *Op. cit.*, 200-202. Aubert, Phys. Opt., 531-536. Rood: *Op. cit.*, 181-194. For measurements of the just observable differences of intensity for different colors, see Helmholtz: *Op. cit.*, G² 402-416; Aubert, Phys. Opt., 531; Fick: *Op. cit.*, 177, and the references given by them. Benson: *Op. cit.*

132. Changes in saturation. Repeat Ex. 131*b*, using colored sectors or stars on white disks instead of black. If star disks are used, it is best to give the rays of the star a leaf shape, for the smaller quantities of color toward the outer ends of the narrow rays fail to

[1] Since the black of the disk is really a very dark gray, this is not an absolutely pure experiment, but is sufficiently exact for the purpose.

make an impression on the white. Notice the paling of the color when mixed with white and the relatively preponderating effect of the latter. Notice also a tendency to change in color tone as well as in saturation, especially when the amount of color is small. Red tends toward rose, orange toward red, indigo toward violet, blue-green toward blue, etc. According to Rood's experiments with the color-mixer, yellow-green and violet are unchanged; Helmholtz with spectral colors gets somewhat different results.

Helmholtz: *Op. cit.*, G[2] 322, 470-471, G[1] 281, F. 369. Aubert: Phys. Opt., 531-532. Rood: *Op. cit.*, 194-201.

133. Size of the colored field. The color sensation is not independent of the size of the retinal area stimulated, if the latter is small; and is also affected by the background against which the small colored area is seen. Paste on pieces of black and white cardboard small squares of several kinds of colored paper, one series 5 mm. square, one 2 mm. square and one 1 mm. square. Walk backward from them and notice their loss of color. Observe also the changes in color-tone.

Helmholtz: *Op. cit.*, G[2] 374-375, G[1] 300, F. 399-400. Aubert: Phys. Opt., 536-539. E. Fick: Notiz über Farbenempfindung, Pflüger's Archiv, XVII, 1878, 152 153.

134. Some Phenomena of Rotating Disks. Talbot-Plateau Law. In several experiments of this section use has been made of rotating disks in studying colors and color combination. All such use depends on the phenomenon of positive after-images (Cf. Ex. 116, *Amer. Jour. Psychol.*, IV, 1891-92, 486). A disturbance set up in the retina does not at once subside, but lasts an instant after the removal of the stimulus. If stimuli follow in sufficiently rapid succession the disturbances are added to one another and fused and the result is the same as though the stimuli had reached the retina simultaneously. The rate of succession necessary to give a uniform sensation is from 25 to 30 per second, the rate depending on the illumination of the disk, the higher rate being required for the greater illumination. When once this uniform sensation has been reached the color and brightness of any given concentric ring of the disk are the same that it would have if all the light reflected from it were evenly distributed over its surface, and no further increase in rapidity produces any effect upon its appearance. This is the Talbot-Plateau law. Rotate a disk like that shown in the cut, increasing the rapidity till the innermost portion gives a uniform gray.

When this occurs, the rate of recurrence in the outer ring is 32 times more rapid than in the innermost, and yet no difference in shade is to be seen. To show that the gray is actually of the same brightness that would come from an even distribution of the light reflected from the whole surface of the ring, look at the disk when at rest through a double convex lens held at such a distance from the eye and disk that no distinct image is formed, but the disk looks an even blur of gray. When the disk is put in rapid rotation the gray remains unchanged.

On rotating disks and their phenomena in general cf. Helmholtz, *op. cit.*, G2 480-501, G1 337-355, F. 445-471. On the Talbot-Plateau law cf. Helmholtz, *op. cit.*, G2 482-483, G1 338-340, F. 446-450. Aubert, Phys. Opt., 515-516.

135. Some Phenomena of Rotating Disks. Brücke's experiment. As the disk used in the last experiment is allowed gradually to go slower and slower, there will be observed in one ring after another, beginning with the inner one, just as it loses its uniform character, a notable brightening. The white sectors now have opportunity to produce their full effect upon the retina before they are succeeded and their impression cut off by black sectors.

Helmholtz, *op. cit.*, G2 481-485, G1 338-341, F. 446-450. Aubert, Phys. Opt., 510.

136. Some Phenomena of Rotating Disks. The Münsterberg-Jastrow phenomenon. *a.* When the disk used in the last experiment gives a uniform gray, pass rapidly before it a thin wooden rod or thick wire, and notice that a multitude of shadowy images of the rod will appear on the disk. The number of images is greatest in the portion of the disk having the most frequent interchange of black and white. *b.* Exchange the disk for one carrying two or more colors. Notice the repetition of the phenomenon, and that the colors of the images are the colors (otherwise completely blended) which the disk actually carries. The explanation of the phenomenon is not altogether clear, but the sudden changes of the background against which the rod is seen seem to have an effect not unlike that of a stroboscopic disk or of intermittent illumination, which would show the rod at rest in its successive positions.

Jastrow: A Novel Optical Illusion. *Amer. Jour. Psych.*, IV, 1891-92, 201-208.

137. Some Phenomena of Rotating Disks. Fechner's colors. Rotate the disk used in Ex. 119 or that used in 134, or indeed almost any black and white disk with a less rapidity than that required to give a uniform gray, keeping the eyes from following the motion of the disk, and notice the play of colors on its surface. These vary with the rate of the disk and the intensity of the illumination. The colors may not at once appear, but are not difficult to get with steady and attentive gazing. The colors owe their existence to an analysis of the light of the white sectors, depending, not on the different wave lengths of the colors, as in the case of the prism, but on the differences in the times at which the different primary color sensations reach their maximum in sensation. The intermittent stimulation causes a rise and fall in the intensities of the fundamental sensations, but the instant of greatest excitation is not the same for all.

Helmholtz, *op. cit.*, G2 530-532, G1 380-381, F. 500-502. Aubert, Phys. Opt., 560.

138. Color-mixing. A general law of color-mixing, and one upon which almost all experiments with artificial colors must depend, may be stated as follows: Like appearing colors produce like appearing mixtures.[1] Thus an orange that is mixed from red and yellow spectral lights will produce the same purple when mixed with violet that spectral orange of like intensity would itself produce. The colored papers with which the experiments below are made are very far from simple colors (as can easily be seen by looking at scraps of them on a black background through a prism), yet they produce the same mixtures that spectral colors of equal tone, intensity and saturation would do. Three colors properly selected

1. This law of course has reference to the mixture of colored lights and not to mixtures of pigments upon the palette.

from the whole range serve to produce by their mixtures all the intermediate colors (though generally in less saturation) besides purple and white (*i. e.*, gray). The colors generally selected are red, green and violet. Green cannot be mixed from colors that themselves do not resemble it, *i. e.*, it can be mixed from yellow-green and blue-green, but not from yellow and blue, and not in anything like spectral saturation. *a.* Mix a yellow from red and green on the color-mixer. The yellow produced will be dark, and as a test of its purity should be matched with a mixture of yellow and black made with smaller disks set on above the first. In the same way mix a blue from green and violet that shall match a mixture of blue and black. *b.* From red and violet or blue, mix several shades of purple between violet and purple. *c.* From red, green and violet, mix a gray that shall match a mixture of black and white on the small disk.

For demonstrational purposes the result of mixing two colors in different proportions can be shown on a single disk of the star form (see Ex. 131) by painting the star in one color and the ground of the disk in another (or by pasting colored papers instead of painting), but in either case some trial will be necessary to determine the proper size for the rays.

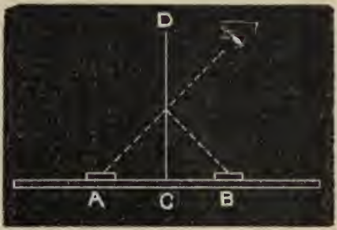
Helmholtz, *op. cit.*, G2 311-316, 320-322, 325-33, 350-357, 375-376, 473, 485, G1 272-277, 279-281, 282, 341, F. 359-365, 367-369, 450. Aubert, Phys. Opt., 521-524, 527-528.

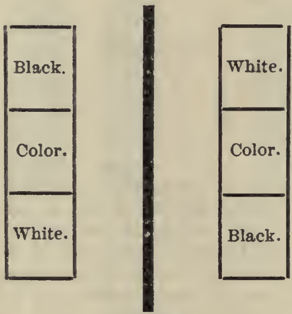
139. Complementary Colors. The combination of red, green and violet mentioned in the last experiment is not the only combination that will give white or gray. For every color there is another or complementary color, which mixed with it will give a colorless combination. Some of these pairs are red and blue-green, yellow and indigo blue, green and purple, blue and orange-yellow, violet and yellow-green. *a.* Try several of these pairs upon the color-mixer, matching the resultant gray carefully with a mixture of black and white on the small disk. It will probably be found in some cases that no possible proportions of the colored papers at hand will give a pure gray. In that case a little of the color complementary to that remaining in the gray must be added. Suppose the red and blue-green papers give, when combined, gray with a tinge of brown (*i.e.*, dark orange), a certain amount of blue or indigo must be added to compensate. For example, with certain papers 180° of blue-green +36° indigo +144° red make a gray that matches 90° white +270° black. To see the true complement of the red used it is then necessary to prepare a disk carrying green and indigo only in the proportions of 180 and 36, *i.e.*, 300° blue-green, 60° indigo. In the same way the complement of the blue-green used is a bluer red than that of the red paper, and may be seen by itself by mixing 288° red with 72° indigo. It is very important here, and in all cases where a resultant white or gray is to be observed, to have some undoubted white or gray in the field to prevent illusions over very faint tinges of color. *b.* Negative after-images when projected on a colorless gray or white surface are seen in colors complementary to those that give rise to the after-images. Compare a pair of complementary colors found in this way with the same pair as found on the color-mixer.

Helmholtz, *op. cit.*, G2 316-319, G1 277-278, F. 365-367. Aubert, Phys. Opt., 521-524. Complementary colors can be well seen with polarized light. See picture of the schistoscope, an instrument for showing them by this means in Rood, *op. cit.*, 162; description of Rose's chromatometer (*Farbenmesser*), Helmholtz, *op. cit.*, F 397; also the Leukoskop, Helmholtz, *op. cit.*, G2 368, and references there given.

140. Other Methods of Mixing Colored Lights. *a.* The simplest of these methods is by reflection and transmission. The colors to

be mixed are placed on a horizontal surface on opposite sides of a vertical glass plate. The eye is brought into such a position that the reflected image of the colored field on the eye side appears to overlie the field on the other side seen through the glass. The glass must of course be of good quality and clean. The relative intensity of the colors can be varied by varying their distance from the glass. Bringing the colors near the glass or raising the eye, strengthens the reflected and weakens the transmitted light. Strips of paper placed with their ends next the glass will show an even blending from a mixture in which one predominates to one in which the other predominates, provided the illumination is equal. To mix two colors in exactly equal proportions, arrange them with black and white, as in the diagram below.





Glass.

Adjust the glass till the grays made by the black and white at the ends exactly match; the colors will then be mixed half and half.

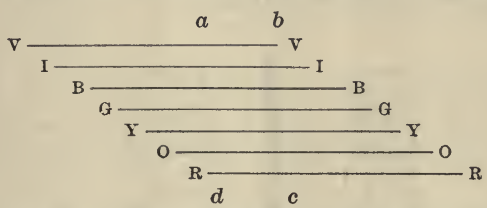
By substituting a bit of glass on a black background for one of the colors and then placing the instrument so that a portion of clear sky may be reflected in the glass, it is possible to mix sky blue with its complement, or with any other color.

b. Colored areas placed side by side can be mixed with the aid of a double refracting prism. The prism doubles both fields and causes a partial overlapping. In the overlapped portion the colors are mixed.

c. Spectral colors can be mixed, though in an inexact way, without more apparatus than a prism and a piece of black cardboard. Fit a piece of black cardboard into the window frame so that it shall cover one pane completely. Cut in the middle of it two narrow slits (1-2 mm. wide and 10 cm. long), meeting each other at right angles and making a broad V. The cuts should be clean and sharp and the slits of uniform width. Look at this V from a distance, 10 or 12 feet, holding the prism vertical. Each arm of the V will give a spectrum, and where they cross, some spots of mixed color may be made out, especially the red and violet giving purple, and red and green giving yellow. The early studies of Helmholtz were

made with apparatus arranged on this principle, but more refined. If lines finer than can conveniently be cut in the cardboard are desired, they can easily be made (after a suggestion of Prof. Pickering) by tracing them on a piece of smoked glass. If the sunlight is allowed to fall on a prism and the spectrum is caught on a white wall or a screen, colors may be mixed with a double refracting prism like the colored fields mentioned above.

d. Something may be done in the way of mixing colored lights with a prism and narrow strips of paper or a diagram like Plate I, or still better, a similar diagram in which black takes the place of white, and *vice versa*. Since a prism refracts different kinds of light to different degrees, it produces a multitude of partially overlapping images of a bright object, which appear to the eye as colored fringes. (Observe through a prism a square inch of white paper on a black background.) These overlapping images may be illustrated by the following diagram, in which the horizontal lines stand for the images and the capital letters for the colors of light producing them.



In the area *a b c d* all the images overlap and the white of the paper is still seen. Toward the left from *a*, however, the different kinds of light gradually fail, beginning with the red. The successive colors from greenish blue to violet result from the mixture of what remains. At the other end a similar falling away of the colors gives the succession from greenish yellow to red. In Fig. 1, Plate I, the spectra seen on the upper and lower edges of the square are brought side by side; on one side red, orange and yellow, and on the other greenish blue, blue and violet. The colors that stand side by side are complementary pairs both in color tone, intensity and saturation; for the greenish blue is the white of the paper less the red, and the blue the same less the red, orange and yellow, and so with the rest; and if the two spectra could be exactly superposed they would make precisely the white from which they originated.

If a very narrow strip of white upon a black ground is looked at through the prism, the images overlap less and another color appears, namely, green, as may be seen in Fig. 2 on the narrow white band between the black bars. When, on the other hand, a narrow black band on a white ground is taken, the spectra of the white surface above and that below partially overlap and give another set of mixtures. If the diagram is held near the prism at first and then gradually withdrawn from it, the advance and mixing of the spectra can easily be followed. Besides the greenish yellow at one end and the greenish blue at the other, there are a rich purple, complementary to the green beside it, and a white between the purple and the greenish yellow. The last is a white produced by the mixture of the blue of one spectrum with the complementary yellow of the other.

In Fig. 3 are shown a number of color mixtures with different proportions of the constituents. In the spectra from the white

PLATE I.

Fig. 1 Fig. 2 Fig. 3

Fig. 4

triangle appear mixtures of each color in the spectrum seen on the white band in Fig. 2, with every other color found there. Upon the black triangle in the spectra from the white edges above and below are seen mixtures similar to those on the black band in the same figure. The diagram should be at such a distance from the prism that a little of the white and black triangles can yet be seen.

On methods of color mixing cf. Helmholtz, *op. cit.*, G2 350-357, 485, 491–493; G1 303–306, 341, 346–349; F. 402–407, 450, 457–461. Aubert, Phys. Opt., 521–524. Maxwell, *op. cit.*, On *a* and *d*, Benson, *op. cit.*

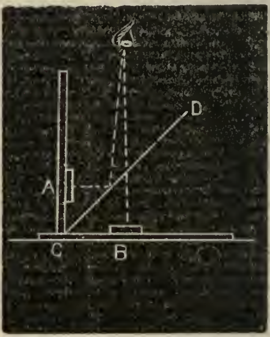
141. Contrast. The effect of one color on another, when not mixed with it, but presented to the eye successively or in adjacent fields, is known as contrast. Two kinds are distinguished, *Successive contrast* and *Simultaneous contrast.* The color that is changed or caused to appear upon a colorless surface, is known as the *induced color*; the color that causes the change is called the *inducing color.* Successive contrast is largely a matter of negative after-images, and their projection upon different backgrounds. Successive contrast: *a.* Prepare a set of colored fields of the principle colors, including white and black, say 3x5 inches in size, and some small bits of the same colors, say 1 cm. square. Lay a small square on the black field, get a strong negative after-image and project it first on the white and then on the other fields. Notice that the color of the after-image spot is that of the field on which it is projected minus the color that produced the spot; *e. g.*, the after-image of red projected on violet looks blue, and on orange looks yellow. Or, to say the same thing in other words, the color of the spot is a mixture of the color of the after-image with the color of the ground upon which it is projected. Thus the blue-green after-image from the red, when mixed with violet, gives blue; when mixed with orange gives yellow. Notice that when the image is projected upon a field of the same color it causes the spot on which it rests to look dull and faded, but when it is projected upon a field of complementary color, it makes the spot richer and more saturated. In general, colors that are complementary or nearly so are helped by contrast, those that resemble each other more nearly are injured in appearance. *b.* These effects in even greater brilliancy can be seen by laying the small square of color directly on the larger colored surface, staring at it a few seconds and then suddenly puffing it away with the breath. Cf. also Ex. 126. *c.* This contrast effect may be so strong as actually to overcome a moderately strong objective color. Place a small piece of opaque orange paper in the middle of a pane of red glass and look through the glass at a clear sky or bright cloud. The strength of the induced blue green will be sufficient to make the orange seem blue. *d.* The contrasting color may even be made to appear upon a surface faintly tinged with inducing color. Rotate one of the disks used in Ex. 132 or Ex. 131*b* rapidly enough to produce an even mixture. Bring the eye within six or eight inches of the disk, stare steadily at the centre for ten or twenty seconds; then suddenly draw back the head. The complementary color will appear to rush in upon the disk from all sides. The explanation is that after the withdrawal of the head the retinal image of the whole of the disk rests upon the part before fatigued by the intensely colored center of the disk.

Helmholtz, *op. cit.*, G2 537–542, G1 388–392, F. 510–515.

142. Mixed contrasts. When special precautions are not taken to exclude successive contrast, both kinds co-operate in the general effect. Some of the results are striking and beautiful. *a.* Colored

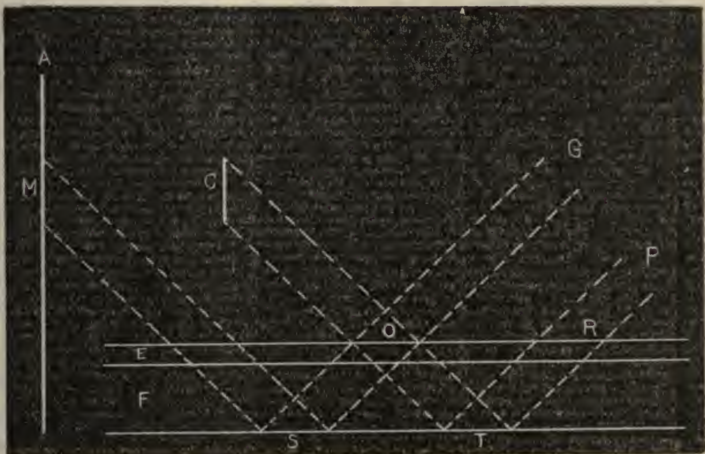
shadows. (1) Arrange two lights so that they shall cast a double shadow of a pencil or small rod upon a white surface, and regulate the brightness (or distance) of the lights so that the shadows shall be about equally dark. The daylight will answer for one light if it is not too strong, but it must come from an overcast sky, for the light from a blue sky is itself blue. Introduce different colored glasses one after another before one of the lights and notice the beautiful complementary color that immediately appears in the shadow belonging to that light. Cf. also Ex. 144. (2) Use a blue glass and adjust the relative intensities of the lights so that the yellow shadow appears at its brightest, and notice that it seems as bright as or brighter than the surrounding blue. As a matter of fact, however, it receives less light than the surrounding portions, for as a shadow it represents the portion of the field from which the light is partly cut off.

b. Mirror contrasts. (1) Ragona Scinà's experiment. Place upon the horizontal and vertical surfaces of the frame described above, white cards carrying black diagrams. Any black spot will answer, but for this experiment diagrams made up of sets of heavy concentric black rings (lines a quarter of an inch wide), separated by white rings of triple width, give an excellent effect. The diameters should be so chosen that a black ring on the horizontal diagram shall correspond to a white one on the vertical and *vice versa*, and shall appear to lie in the midst of the white when the diagrams are combined in the way immediately to be described. A pair of diagrams made up of parallel black bars, a quarter of an inch wide, separated by quarter inch spaces, and so placed in the instrument that they give a checker-board pattern when combined, are useful for keeping in the field a true black with which the changed colors can be compared. The diagrams being in place, hold between the two at an angle of 45° a pane of colored glass, say green, and observe that the black of the horizontal diagram seems tinged with the complementary color, that is, purple. This contrast color may often be improved by slightly altering the inclination of the glass, or by changing the relative illumination of the diagrams by interposing a colorless screen between one or the other of them and the source of light, or by shifting the whole instrument. The mechanism of this experiment will be readily understood after a consideration of the accompanying cut. The glass plate is represented by $C\,D$, the black portion of the vertical diagram by the projection opposite A, that of the horizontal diagram by the projection at B. The light reaching the eye from the white portion of the horizontal diagram is colored green by the glass; that from the white portion of the vertical diagram is reflected from the upper surface of the plate, and is therefore uncolored[1]. The mixture of the two gives a light green field. For simplicity, we may assume that no light comes from the black portions of the diagram. In the portion of the light green field corresponding to the black of the vertical diagram, the white component will be wanting and the green will appear undiluted; in the portion cor-



[1] A small portion is also reflected from the lower surface of the glass, so contributes a small amount of green.

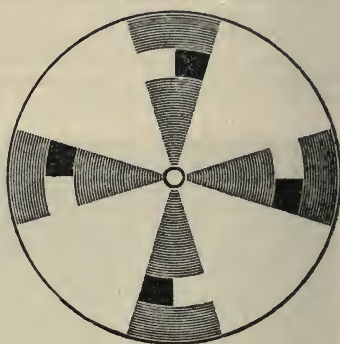
responding to the black of the horizontal diagram, the green component will be wanting and the faint white (*i. e.*, gray) should appear by itself. It does not, however, because of the contrast color induced upon it. As a matter of fact, the black portions are not absolutely black; the small amount of light that comes from them tends on one hand to make the green one (image of the black of the vertical diagram) a little whiter, and on the other hand to counteract the contrast in the purple one by adding to it a little green. Try the experiment with other glasses than green. (2) Another form of the mirror contrast experiment is as follows. Place a mirror where the sky or a white surface of some kind will be seen reflected in it. Lay upon its surface a plate of colored glass, green for example, and hold a little way above it a narrow strip of black cardboard or a pencil. Two images will be seen: one a vivid green, the other a complementary purple. The green image belongs to the surface reflection of the colored glass, as may be proved by observing that when the strip of cardboard touches the surface, the green image touches it also. The explanation will readily be understood from the accompanying diagram.



In the diagram, *A* represents a white surface, *C* the strip of cardboard, *E* the plate of green glass, *F* the glass of the mirror. As in Ragona Scinà's experiment, the white surface is reflected unchanged from the upper surface of the green glass. A good deal of the light, however, traverses the green glass and the mirror, is reflected from the back of the mirror, traverses the green glass again, and finally, as a strong green, mixes with the white reflected from the surface of the green glass, forming, as in Ragona Scinà's experiment, a light green field. The black strip *C* is reflected at *O*, that is to say, at *O* is a place where the white from the surface *A* is cut off, and only green from *M*, by way of *S*, is present, hence its image appears green. But *C* is also reflected at *T* (or its light is wanting there), so that the white reflected from *R* is unmixed with green. By contrast, it appears purple. It is easy, by substituting for *C* a gray strip that will send some light through the glass at *O* and *R*, to show that contrast can suppress an actually present objective color.

c. Meyer's experiment. Lay on a large colored field a small piece of gray or even black paper (*e. g.*, 1 cm. wide by 2 cm. long), and cover the whole with a piece of semi-transparent white paper. The contrast color will appear on the gray paper. If thin tissue paper is used, more than one thickness may be needed for the best result. R. Jung, Heidelberg, sells a book (for Becker's *Florversuche*) of alternate leaves of colored and tissue paper, with two gray rings attached, made expressly for Meyer's experiment. Paper mats, woven one way of gray paper and the other of colored, show this contrast beautifully, as Hering mentions. They may easily be made from kindergarten materials.

d. Mixed contrasts with the color-mixer. (1) Disks made on the pattern of the cut at the left show beautiful contrasting grays.



The same can be shown also by laying a number of small sheets of tissue paper over one another in such a way that they partially overlap, making a portion where there is but a single thickness, and next it a portion where there are two thicknesses, and next that again one of three thicknesses, and so on. When the whole is held up to the light, the contrasts of adjacent portions are very easily seen. (2) Contrast colors can be shown finely with disks like that in the cut at the right, in which the shaded portions represent color, the black portions black and the white, white. A little care is necessary in fixing the proportions of the color to white and black in the disks for contrast colors, but in general the brightness of the gray should be about that of the color.

On *a* cf. Helmholtz, *op. cit.* F. 517-519, 531; G.[1] 393-395, 405; G.[2] 551-553. Hering: Ueber die Theorie des simultanen Contrastes von Helmholtz; Die farbigen Schatten, Pflüger's Archiv, XL, 172. V. Bezold, *op. cit.*

On *b* (1) cf. Helmholtz, *op. cit.* F. 531-532; G.[1] 405-406; G.[2] 557-558. Hering: Ueber die Theorie des simultanen Contrastes von Helmholtz; Der Spiegelcontrastversuch. Pflüger's Archiv, XLI, 1887, 358-367. Wundt, *op. cit.*, I, 482. See also the physiologies in general.

On *b* (2) cf. Dove: Versuche über subjective Complementarfarben, Pogg. Ann., XLV, 1838, 158. Helmholtz, *op. cit* F. 532, G.[1] 406; G.[2] 558. V. Bezold, *op. cit.*

On *c.* Helmholtz, *op. cit.* F. 523, 530-531; G[1] 398, 404 405; G.[2] 547-548. Hering: Ueber die Theorie des simultanen Contrastes von Helmholtz; Der Contrastversuch von H. Meyer und die Versuche am Farbenkreisel, Pflüger's Archiv, XLI, 1887, 1-29.

On *d.* Helmholtz, *op. cit.* F. 538-543; G.[1] 411-414, G.[2] 544-547. Hering, *op. cit.* on *c.* V. Bezold, *op. cit.* Meyer, Pogg. Ann., XCV, 170, Phil. Mag., Ser. 4. IX, 547.

For quantitative measurements of contrast of grays cf. Ebbinghaus, Die Gesetzmässigkeit des Helligkeitscontrastes, Sitzber. d. k. Preus. Akad., Berlin, Sitz. v. 1, Dec., 1887. Lehmann: Ueber die Anwendung der Methode der mittleren Abstufungen auf den Lichtsinn; Die quantitative Bestimmung des Lichtcontrastes. Wundt's Philos. Studien, III, 1886, 516-528.

143. Conditions that influence contrast. *a*. Contrast effects are stronger when the colors are near together. (1) Lay a bit of white paper on a black surface, *e. g.*, a piece of black velvet, and notice that the paper is whiter and the velvet blacker near the margin of the paper than elsewhere, notwithstanding that the eye moves about freely. This has received the name of "Marginal contrast" (*Randcontrast*). (2) On a piece of gray paper, the size of a letter-sheet, lay two strips of colored paper close side by side (*e. g.*, pieces of red and yellow or of green and blue, 1 cm. wide by 4 cm. long). Below them to the right and left as far apart as the paper will permit, lay two other strips of the same size and color, red on the red side of the former pair, yellow on the yellow side. Notice the effect of the difference in distance on the contrasting pairs. Contrast of this sort is at a maximum when one color entirely surrounds the other.

b. Effect of size. When the area of the inducing color is large and that of the induced color is small, the contrast is shown chiefly on the latter; when the two areas are of about equal size, as in *a* (2) above, the effect is mutual. Try with large and small bits of paper upon a colored field.

c. Borders and lines of demarkation that separate the contrasting areas tend to lessen the effect by excluding marginal contrast. Repeat Ex. 142*c*, using two slips of gray paper 5 mm. wide by 2 cm. long, substituting a piece of moderately transparent letter paper for the tissue paper. When the contrast color has been noted, trace the outline of one of the slips with a fine ink line upon the paper that covers it. Notice that the color nearly or quite vanishes. This experiment and others like it play an important part in the psychological, as opposed to the physiological, explanation of simultaneous contrast (see Helmholtz, *op. cit.* F. 533 f., G.[1] 406 f., G.[2] 559 f., but cf. also Ex. 144). Such a black border will, however, also make a weak objective color invisible. A disk like that in the cut accompanying Ex. 142*d*, when provided with a second contrast ring, marked off on both its edges with a firm black line, shows a weakening of the induced color in the bordered ring.

d. Saturation. Contrast effects are generally most striking with little saturated colors. (1) Compare the effect of increasing, decreasing and extinguishing the second non-colored light in the colored shadow experiments. It is necessary, however, to see to it that reflected light from the walls and surrounding objects does not complicate the experiment. (2) Compare the intensity of the contrasts in Meyer's experiment (Ex. 142*c*) before and after the application of the tissue paper. Notice also the part played by the white light mixed with the colored light in the mirror contrast experiments above. Powerful contrasts with the most saturated colors can be observed, however, when the proper conditions are fulfilled.

On helpful conditions in general cf. Helmholtz, *op. cit.* F. 513-514, G.[1] 390-391, G.[2] 540-541.
On *c*. Helmholtz, *op. cit.* F. 539-542, G.[1] 411-414, G.[2] 546-547.
On *d*. Helmholtz, *op. cit.* F. 523-524, G.[1] 399-400.

144. Simultaneous contrast. The effects of simultaneous contrast are often lost in the more striking ones of successive contrasts, and the first requisite of an experiment on simultaneous contrast is the exclusion of the successive. This is not difficult in experiments in colored shadows. *a*. Place a piece of white paper in such a position that it may be illuminated at once from the window (if the day be overcast) and from a gas-jet. Set upon it a small block or other object, about 1½ by 3 inches in size, and either black or white

in color. Light the gas and observe the two shadows, one cast by the light from the window, the other by the gas. The first will appear yellowish, the second clearly blue.[1] Adjust the distance and position with reference to the light so that the shadows shall appear about equally dark, and the blue shadow shall be as sharply bounded as possible, and to that end have the shadow cast by the edge rather than the flat side of the flame. The color of the yellowish shadow is objective and due to the yellow of the gas-flame, that of the blue is due to the contrast, but largely as yet to successive contrast. Put a dot in the centre of the blue shadow, to serve as a fixation-point, and another on the edge. Fasten a paper tube (preferably blackened inside) so that it can easily be shifted from one dot to the other. Cut off the gas-light by holding a card between it and the block; adjust the tube so that the dot in the middle of the shadow may be fixated without any of the parts of the field outside of the shadow being seen. Wait until all of the blue has disappeared from the shadow, and then, still looking through the tube, remove the card. The field remains entirely unchanged and appears, as before, a colorless gray. The former blue color is thus shown to be subjective and due to contrast with the yellow lighted area in which it lies. *b.* Cut off the gas-light again and adjust the tube so that the dot in the edge of the shadow may be fixated. Taking great precaution not to move the eye, withdraw the card. The part of the field of the tube filled by the shadow will appear bluish, that of the remainder reddish-yellow. After a little time of steady fixation, cut off the gas-light once more and observe the instant reversal of the colors. The shadow now appears in reddish-yellow, the rest of the field blue. The color of the shadow, both before and after the final interposition of the card, is due to simultaneous contrast, in the first case with the reddish-yellow light, and in the second with its after-image.

Helmholtz explains all cases of simultaneous contrast as errors of judgment; in the case of the colored shadow, for example, we mistake the yellow of the gas-lighted field for white and consequently find the shadow which is really gray to be bluish. In the case of this particular experiment, Hering and Delabarre seem to have shown this psychological explanation unnecessary and a physiological one all sufficient, and Hering has done the same for other forms of experiments.

Cf. on simultaneous contrast in general. Helmholtz, *op. cit.* F. 515-547, G.[1] 392-418, G.[2] 542 ff. Hering, *op. cit.*, under Ex. 142. On colored shadows, cf. Helmholtz, *op. cit.* F. 517-519, G.[1] 394-396, G.[2] 551-553.

On Helmholtz's theory, cf. Helmholtz, *op. cit.* F. 533-538, G.[1] 392, 407-411, G.[2] 543 ff. Hering, *op. cit.* under Ex. 142 ; also, Ueber die Theorie des simultanen Contrastes von Helmholtz : Die subjective " Trennung des Lichtes in zwei complementäre Portionen."

Delabarre : Colored Shadows, AMERICAN JOURNAL OF PSYCHOLOGY, II, 1888-89, pp. 636-643. For quantitative measurements of simultaneous contrast, see Kirschmann: Ueber die quantitativen Verhältnisse des simultanen Helligkeits und Farben-Contrastes, Wundt's Philos. Stud., VI, 1890.

145. Simultaneous contrast. Hering's binocular method. *a.* Use the binocular color-mixer described above in the note on apparatus. Set a red glass in the right frame, a blue glass in the left. Look fixedly through the colored glasses of the instrument at the cork ball below, bringing the eyes close to the glasses and the

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[1] This setting of the experiment succeeds best when the daylight is weak, as, for example, just before the lights are usually lighted in the evening. If the experiment is to be made in broad day, the light must be reduced by curtains or otherwise ; if at night, there must be two lights, one corresponding to the window and one to the gas, and the latter must shine through a pane of colored glass. If yellow glass is used the colors will be the same as those in this experiment.

nose between them. Adjust the side screens till the white ground below appears in a uniform light violet from the binocular mixture of the red and blue (cf. Ex. 153). The narrow strip of black paper on the white is seen double, the right hand image bluish, the left yellowish. *b*. The possibility of successive contrast is, however, not yet excluded. That may be accomplished as follows: Lay a sheet of black paper over the whole of the white field and its black strip; rest the eyes; and finally, when everything is in readiness, and the eyes again fixed on the ball, swiftly draw away the black paper. The contrast colors are seen on the instant, before any motions that might introduce successive contrast have been made.

Hering argues that this experiment is conclusive against the psychological explanation of simultaneous contrast, unless a separate unconscious judgment is to be made for each eye, for that which consciously appears is a light violet field, and the contrast color to that should be a greenish-yellow, and both images of the strip should be alike, whereas, as a matter of fact, the images appear in different colors, neither of which is the color required.

Hering: Beitrag zur Lehre vom Simultankontrast, Zeitsch. f. Psychol. I, 1890, 18-28. For a different experiment supporting the same conclusion, see Hering's paper.

146. Influence of judgment in visual perception. While in the previous experiment a psychological explanation seems sufficient for the facts, psychical action is not excluded, even by Hering, from a considerable share in sense perception. In the following experiments judgment coöperates in the result. *a*. Place upon the color-mixer a short-pointed star of white cardboard, or even a square, when in sufficiently rapid rotation, it appears as a white central circle surrounded by a more or less transparent ring. While it is in rotation bring behind it a broad strip of black cardboard of somewhat greater length than the diameter of the star from point to point. As the edge of the card advances it can be seen not only behind the transparent ring, but, apparently, also behind the opaque central circle, and the portions of the latter in front of the black card seem darkened by its presence. The illusion holds, though with a lightening instead of a darkening effect when a white card is moved behind a black star. The illusion fails by degrees if the card is kept motionless, but may be observed to a certain extent when the star is at rest. *b*. Cover a piece of black cardboard smoothly with tissue paper and notice that it seems, at first, blacker than it afterwards proves to be on comparison with other grays. *c*. In mixing colors by reflection (Ex. 140*a*), notice the tendency to see one color through the other instead of seeing the mixture of the two. This tendency may be so strong at first as to interfere, to a certain extent, with the success of the experiment. Cf. also Ex. 152.

On the difficulty of judging small differences in the color of surface that present other small unlikenesses. cf. Hering, *op. cit.* under Ex. 142*c*.
On *a*., Sanford, *Science*, XXI, 1893, 92.

147. An effect exactly the reverse of contrast appears when a figure in black or white is placed upon a colored ground. The black figure appears to darken the ground and the white to brighten it. This is a method often used in polychromatic decoration. Observe the effect on the blue ground in Fig. 4, Plate I. It may be observed occasionally in plaid fabrics, and is shown very satisfactorily in kindergarten mats woven in checker-board pattern of colored and gray papers. If a set of grays is used so that the strips may range from a black at one side to a white at the other, the corresponding shading of the colored paper is striking.

V. Bezold, *op. cit.* 182-183, and Plate V.

BINOCULAR PHENOMENA OF LIGHT AND COLOR.[1]

148. In general the two eyes coöperate to bring about a single visual result, but the union of the impressions upon the two retinæ is influenced by a number of circumstances. *a.* If the stimulus to one eye is considerably stronger than that to the other, the sensation in the latter is in most cases totally suppressed. Close one eye and look at a sheet of white paper with the other, letting the open eye move about freely. There is no tendency for the darkened field of the closed eye to assert itself. *b.* When, however, the effect of the stimulus in the open eye is somewhat weakened by steady fixation, such a tendency is to be observed, and the whole of the field of the open eye, except a small area about the point fixated, may be, from time to time, suppressed by the dark field of the closed eye. A slight motion will, however, instantly restore the first. Cf. also Ex. 118. *c.* A field that contains sharply marked objects or contours will generally triumph over one that does not. Try combining the letters below in such a way that the B's are superposed. In these the white field of either eye which corresponds to A or C in the other eye will generally not triumph over the letters.

A B   B C

On the Binocular Phenomena of Light and Color in general, see Fechner: Ueber einige Verhättnisse des Binocularen Sehens, Abhandlungen der kgl. sächs. Ges der Wiss. VII. 1860, pp. 339 564. Helmholtz, *op. cit.* F. 964-999, G.[1] 767-796. Hering: Hermann's Handbuch der Physiol., III, Th. i, 380 385, 576-577, 591-601 ; Beiträge zur Physiologie. 308-316. Aubert: Grundzüge der physiologischen Optik, 499-503, 550-554. Wundt: Physiol. Psychol. 3te. Aufl. II., 177-179, 183-189. Ebbinghaus: Ueber Nachbilder im binocularen Sehen und die binocularen Farbenerscheinungen überhaupt, Pflüger's Archiv, XLVI, 1890, 498-508. Titchener, Ueber binoculare Wirkungen monocularer Reize, Wundt's Philos. Studien, VIII, 1892, 231-310. Chauveau: Several articles in the Comptes rendus, CXIII, 1891, 358, 394, 439, 442.

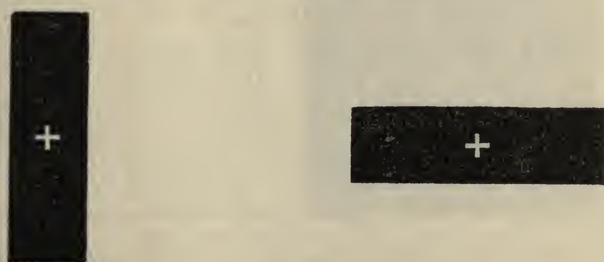
149. Fechner's paradoxical experiment. Hold close before one eye a dark glass, such as is used in protecting the eyes, or a piece of ordinary glass moderately smoked over, or even a black card with a good sized pin-hole in it, allowing the other eye to remain free. It is easy to see that the binocular field is darkened by the interposition of the dark glass. If, however, the eye behind the glass is closed, or the light wholly cut off from it by holding a black card in front of the glass, the field appears decidedly brighter, that is to say, cutting off a portion of the stimulus received by the total visual apparatus, has caused an increased intensity of sensation. The experiment fails for very dark and very light glasses. Several explanations have been given, but that of Aubert, according to which the sensations of the two retinæ blend in a sort of average result when the difference is not too great, but one wholly suppresses the other when it is very great, seems to be the most satisfactory, and with this Hering also in the main agrees.

150. Rivalry. When the two retinæ are stimulated separately with strong light of different colors, or are confronted with otherwise incongruous fields, *i. e.*, fields that cannot be given a unitary

[1] The experiments that follow can all be made with the stereoscope, but practice will enable the experimenter to combine the diagrams with free eyes, either by crossing the lines of sight (fixating a point nearer than the diagram), or by making them parallel or nearly so (fixating a point beyond the diagram). This skill the experimenter should try to acquire. In these experiments it is important that the eyes should be of approximately equal power, and if the poorer eye cannot be helped with lenses, the vision of the other must be somewhat reduced by the interposition of a sufficient number of plates of ordinary glass.

interpretation, there result a peculiar instability and irregular alternation of the colors over part or the whole of the combined fields of vision. This apparent struggling of the fields is known as *Retinal rivalry*. Hold close before one eye a piece of blue glass, before the other a piece of red glass, and look toward the sky or a brightly lighted uniform wall. The struggle of colors will at once begin. The same may be observed with a stereoscope when the usual paired photographs are replaced by colored fields, or even with no apparatus at all, when both eyes are closed and turned toward a bright sky and one of them covered with the hand. Rivalry has been explained as due to fluctuations of attention, and some observers find that it can be more or less controlled by attention (Helmholtz); Fechner discusses the attention theory, and finds it insufficient. Hering and others regard the changes as of a purely physiological origin. Cf. Ex. 151*b*.

151. Prevalence and rivalry of contours. By "contours" is here meant lines of separation where fields of one color border upon fields of another color. *a*. Combine stereoscopically the two bars below, and notice that it is the contours that suppress the solid parts of both the black and white. This figure gives excellent results when colors are substituted for the black and white.



Notice a similar triumph of the contours of the cross over the lines, and of the lines over the central black of the cross in Fig. XI, or an enlargement of it.

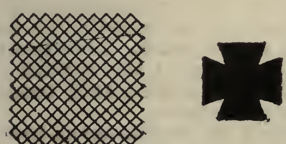


Fig. XI

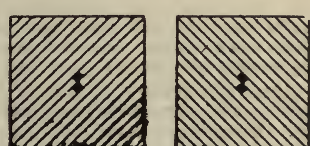
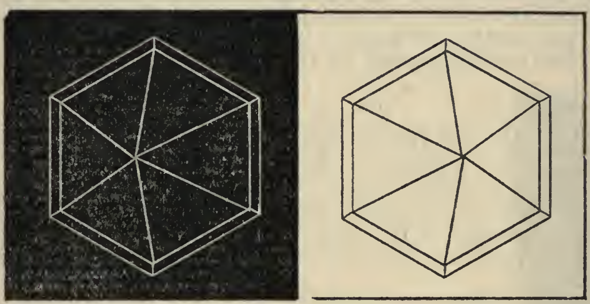


Fig. XII

b. Notice the rivalry of the contours in all of these figures. *c*. Such diagrams as Figs. XI and XII are suitable for the study of the part played by attention in rivalry. While it is doubtful that mere attention to one field or the other will cause it to predominate, it yet seems possible by indirect means to cause it to do so. If attention is given to an examination of the lines and small squares in Fig. XI, or if one of the series of lines in Fig. XII is counted, they will appear to be somewhat assisted in their struggle with the cross or the other set of lines. *d*. A printed page has a decided advan-

tage. Try a diagram in which a printed page is brought into competition with a field of heavy cross lines. The lines will be found to yield to the print, at least, at the point at which the reader is looking. Two printed pages, however, become hopelessly mixed, and it is hard to say how much of the advantage, when a single one is used, is due to its superior power as a holder of attention, and how much to its excellence as a set of contours. A portion of the power of contours is probably to be explained by the mutual intensification of both the black and the white by contrast, but a part is perhaps due to a strong tendency, observable in other cases also, for the eyes (and attention) to follow lines, and especially outlines.

152. Luster. When one of the rival fields is white and the other colored (especially when one is white and the other is black), there results, besides the rivalry, a curious illusion of shine or polish, known as *Binocular lustre*. *a*. Examine in the stereoscope a diagram made like the accompanying cut, and notice the graphite-like shine of the pyramid. The explanation seems to be that polished sur-



faces, which at some angles reflect light enough to look white, and at others appear in their true color, have often in previous experience given rise to such differences of sensation in the two eyes, from which in this instance we infer a polish on the object seen in the diagram. *b*. A species of monocular lustre (or transparence) is to be observed when black or white or colors are combined by means of the reflection color-mixer, especially when the inclination of the plate is so changed that one color appears to be reflected in the surface of the other, or to be seen through and behind it. The experiment works well when real objects are reflected in the surface of the glass, the reflecting power of the latter appearing as if transferred to the horizontal surface on the opposite side.

153. Binocular color-mixing. The result of simultaneous presentation of different colors to the two eyes is not always rivalry or lustre. If the colors are not too bright and saturated and the fields are without fleck or spot to give one the predominance, a veritable though somewhat unsteady mixture of the colors may result. *a*. Place a red and a blue glass of equal thickness in the binocular color-mixer, and adjust the side screens till the proper amount of white light is mixed in with that transmitted from below. The mixture will be seen on the white field below. Try also with other combinations of glasses. The mixtures obtained in this way are not exactly the same in appearance as the monocular mixtures studied above. *b*. The same effect may be conveniently obtained with a stereoscope, from which the middle partition has been removed.

Try with equal areas of dull colors of little saturation. Hering recommends two squares of red and two of blue, set at equal distances in a horizontal line, the two reds on one side, the two blues on the other. When the middle pair are combined stereoscopically, they show a mixed color, while the unmixed colors can be seen for comparison beside them. He also suggests the use of lenses to prevent sharp focusing of the eyes upon the contours, which interfere with the mixture. Complementary colors are said to be more difficult to fuse than those standing nearer in the color scale. Cf. also Ex. 149. For diagrams that bring in binocular perspective to aid in mixing the colors and for a specially adapted stereoscope, see Chauveau.[1]

154. Binocular contrast. The side-window experiment. Stand so that the light from the window falls sidewise into one eye, but not at all into the other. Place in a convenient position for observation a strip of white paper on a black surface. The paper when looked at with both eyes appears perfectly colorless. On looking now at a point nearer than the bit of paper (*e. g.*, at the finger held up before the face), double images of the bit will be seen. The two images will be different in brightness and slightly tinged with complementary colors. The image belonging to the eye next the window (which may be recognized by its disappearance when that eye is closed) will appear tinged with a faint blue or blue-green color, the other with a very faint red or yellow. The light that enters the eye through the sclerotic is tinged reddish-yellow, and makes the eye less responsive to that color; the white of the paper strip therefore appears bluish. It appears darker partly for a similar reason, and perhaps also, as Fechner suggests, because it lies in a field which for the eye in question is generally bright. The reddish color of the other eye's image of the strip is explained as due to contrast with the first, but whether this contrast is a purely psychical matter, or whether it is to be explained by the action of the stimulus in the first eye upon the second, as there seems some reason to think, is as yet uncertain. Its greater brightness is probably due to the fresher condition of the eye to which it belongs, and to contrast with its less brilliant field. The same thing is often to be noticed when reading with the lamp at one side, or even when one eye has been kept closed for a short time while the other has been kept open. The double images are in nowise essential; simple alternate winking will show decided differences in the condition of the two eyes.

155. Binocular after-images. Lay a bit of orange-colored paper on a dark ground, and provide two white cards. Hold one of the cards close to the left eye, but a little to one side, so as *not* to hide the bit of paper. Hold the other eight or ten inches from the right eye in such a way as to hide the paper. Look at the paper for a few seconds with the left eye, then bring the card before it. A faint, washy, orange-colored positive after-image will appear on the card before the right eye. This after-image is supposed to belong to the right eye's half of the visual apparatus, possibly to the central, *i. e.*, cerebral, part.

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[1] Comptes rendus, CXIII, 1891, p. 442.

NOTES AND LITERATURE.

NOTE ON COLOR-HEARING.

In 1887 I met a remarkable case of color-hearing and made quite full notes. The matter was one of which, at that time, I had never heard, and thinking I had a new field for study, I went hunting for victims, subject to pseudo-chromesthesia. I studied some blind cases and had a theory. Just then I discovered Galton's Study upon the same subject and dropped the matter. I made a second observation upon my original subject in 1891, and this spring made a third one, and believe the case of sufficient interest to make a record of it.

I have no theory to offer. I do not agree with Mr. Galton's, however, but consider one of the physiological explanations preferable. When I first studied the subject, I believed that the phenomenon was due to crossing or mingling of fibres of the auditory and visual nerves—a theory which I see has been held by some writers. I am not qualified to urge the theory or reject it; but I am heartily thankful to Mr. Krohn for his late valuable synopsis of the subject, and its literature.

G. L. is a young lady of unusually quick and bright mind; she has some artistic power and is a writer of ability. From early childhood she has had color-hearing. When a very little girl she was laughed at because she said that names were colored. That in her case it is truly sound that is colored is shown by the fact that names were colored before she could spell or read at all. At the same time she has mental imagery of great clearness and now *sees* the colored words when she hears them. Although all words are colored, letters, numbers and personal names are the most clearly and definitely affected. In my first test I gave many words like snow, sky, ink, grass, and the like, to see if natural quality of objects had influenced the result, but could not see that it had. Unfortunately different lists were used on the three occasions, and only letters, numbers and a few personal names occur in all these.

The letters of the alphabet bear the following colors for G. L.:

A. Dirty-white.
B. Radish-color.
C. Corn-color.
D. Dark-brown.
E. Cloudy-white or light-gray.
F. Light-gray.
G. Grayish-yellowish-white.
H. Dark-gray, greenish tinged.
I. Dirty-white.
J. Dark-greenish-bluish-gray.
K. Gray.
L. Yellow.
M. Geranium-red.
N. Red, with white tinge.
O. Pearl-white.
P. Deep-bright-green.
Q. Light-gray-green.
R. Rich-reddish-brown.
S. Sunshine-yellow.
T. Blue.
U. Gray-white.
V. Radish-color and brown.
W. Red, with white cloud.
X. Brownish-beet-color.
Y. Dirty-white.
Z. Deep-beet-color.

This list is that of 1893. Comparing it with the earlier ones, I find no important differences. The only real variants are as follows:

1887.	1891.
D. Dark-brown.	H. Dark-stone.
G. Pearl.	Q. Light-green, yellow-edged.
K. White, edged gray.	V. Pinkish-terra-cotta.
Q. Greenish-yellow.	Y. Yellowish-gray.
V. Burnt-sienna and yellow.	
Y. Yellowish-white.	
Z. Pinkish, dirty-dark-mottled-brown.	

We shall not attempt an analysis of the case, but will call attention to a few points. The vowels are all gray or white; M and N are almost alike; L and S are identical.

As to numbers:

1. White.
2. Blue.
3. Radish-color.
4. Brownish-radish-color.
5. Dark-gray-green.
6. Dark-rich-green.
7. Sunshine-yellow.
8. Cadmium-yellow.
9. Dark-dull-gray-green.
10. Pale-sunshiny-yellow.
11. Corn-color.
12. Corn-color, deeper-edged.
13. Beet-color.
14. Beet-color faded.
15. Dark-green-gray.
16. Dark-green.
17. Light-sunny-yellow.
18. Richer-deep-yellow, brown haze.
19. Dark-dull, gray, green.
20. Pale-gray-blue.

The variants are of little consequence:

1887.	1891.
5. Black, white-edge, misty.	10. Golden, much like 7.
10. White, yellow suffusion.	13. Radish-color.
15. Dark-gray.	17. Yellow, "not so glorious as 7."
20. Cadet-blue.	18. "Almost old-gold."

The radish-color of 3 is identical with that of B. The sunshine-yellow of S and L is that of 7 and 8. Notice in Alice later it recurs; it is plain that the L sound prevails. The influence of the first part of all the "teens" is plain in the above list.

Long lists of names were given in each test, and to all the answers came immediately and definitely. Forty-one names occur in all three lists. Of these only five can be considered variable at all. They are as follows:

1893.	1891.	1887.
Nancy—Dull-red.	Dark-dirty-radish.	Whitish-brown.
Susan—Bright-blue, (with yellow).	Radish (with yellow).	Blue (with yellow).
William—Faded-light-red.	Gray (buff tint).	White (red edged).
Clifford—Light brown (and corn).	Light-gray (and buff)	Gray.
Moses—Red (yellow lining).	Pale-red(and yellow)	White and gray.

Out of the forty-one, Moses is the only real disagreement. Notice how sounds combine and control. The N in Nancy is clearly

the red or brown foundation. S gives a yellow always in Susan. The yellow lines for S in Moses appear on the red ground of M. We have not room of course for the whole name list, but here are some of the simpler cases:

Mamie—Yellowish-red.
Julia—Radish-color.
Minnie—Light-red.
Laura—Light-yellow.
John—Vandyke-brown.
Henry—Gray-blue.
Harry—"A bright luminous blue."

Fringes, crosslines, cloud tints occur, and always interestingly.

Mary—" Pearl white, suggestive of red lines."
Jane—" A deep sombre uncompromising green."
Caroline—" Thin but clear deep, bright blue, showing sunshine lines behind."
Frederick—" Brown made lighter and colder by white."
Moses—" Red, with fine lines of yellow."
Joseph—" Gray, with slight, fine lines of corn-color and black."

It is frequently in these detailed cases that the agreement between different observations is the most striking.

In closing we desire to add a few notes made on blind subjects at the State College for the Blind, Vinton, Iowa, in 1887. We seemed to find among the congenital blind absolutely no conception of color, and no tendency to imagine it in terms of other sensations. On the other hand we believe that those who have become blind in childhood, after learning colors, very soon come to perceive color sensations in their hearing. We examined perhaps a dozen. Of these, three were born blind, or were blind from infancy. Of the rest, at least three had color hearing to a marked degree. The most interesting case was Miss N. This lady had pseudo-chromesthesia before she became blind. Musical notes are colored to her:

Do—Brown.
Re—Gray.
Mi—Light-red.
Fa—Black.
Sol—Real white.
La—Dark-red.
Si—Light-clear-yellow.
Do—Lighter brown than lower *Do*.

The alphabet is colored, also numbers. In her case color is plainly associated with a mental *picture*, for 10 is colored white (1) and black (0), and 11 is very white. So words and names are colored from their component letters. Ink is white (I), green (N), yellow (K). The tones of musical instruments are colored. Thus: the

Violin—Red.
Clarinet—Reddish-yellow.
Piano—Low notes, black; high notes, clear, like crystal or yellow glass.
Guitar—Blue-purple.
Flute—" Diamond clear."
Harp—Brown and blue.
Banjo—" Almost like violin, but darker."

Voices are distinctly colored: my own was " brown and gray, real light," while my little boy companion spoke in " real dark brown " tones. The months are colored and I could not detect any association influence from sky or weather. The color-sound and mental-imagery of the blind is, I am convinced, an interesting special field of study.

PROF. FREDERICK STARR.

The article in Vol. V, No. 2, of this JOURNAL concerning the experiments conducted under Prof. Jastrow's direction in reference to our estimation of verticals and angles, leads me to think that an observation of my own, which I have not seen recorded by any writer, may not be without interest to the readers of this JOURNAL.

Let the experimenter prepare a large sheet of paper thus: Set this in some convenient position, with its lower line horizontal against a dark background, the paper itself being well illuminated. The observer's eyes themselves should be screened from strong light. Let the observer lie down, resting on the left side, facing the paper, the plane of which must be about perpendicular to the line of vision.

FIG. 1.

Let him gaze at the black spot steadily for as long as may be necessary to obtain a strong *after-image.* This will be *thought* as shown in Fig. 2.

Let the observer now, with eyes still closed, roll over to the right; he will find that he *thinks* the image turning on an axis in the same direction, and when he reaches a position in which he is lying on his back he will *think* the position of the image as seen in Fig. 3.

If he turns still further until he is lying upon his right side, having made a ½ revolution, he will *think* the image as presented in Fig. 4, it having made only a ¼ revolution.

FIG. 2.

FIG. 3.

FIG. 4.

If he turn with face down he will think the image as seen in Fig. 5.

If he turn one more quarter, one would suppose from what has preceded that he would think the image as shown in Fig. 6; but in fact somewhere between Figures 4 and 5 there is a change, and when he reaches the end of this last ¼ turn the image is thought as it was when he originally lay in this position on his left side. See Fig. 7.

FIG. 5.

FIG. 6.

FIG. 7.

This simple experiment seems to me to indicate that our judgments of verticality are determined largely by relation of our retinal images with the consensus of the feelings involved in balance or relation of our whole mass to the direction of the force

of gravity. How enormously complex must be the judgment-problems here involved, is apparent when we consider that each difference of position involves changes in some elements of the complex psychosis of balance.

I present this simple fact to the active workers in the field of experimental psychology, hoping that it may prove of service to those who are engaged in the study of these special phenomena.

HENRY RUTGERS MARSHALL.

Zur Physiologie und Pathologie der Handschrift. GOLDSCHNEIDER. Archiv für Psychiatrie, XXIV, 1892, 503.

An important contribution to the theory of hand-movements. G. gives, first, a theoretical account of the origin of what the present writer has called "tracery-imitation," under the equivalent phrase, *malende reproduction*, *i. e.*, the association between visual pictures (letters, figures, etc.) and the hand-movements necessary to reproduce them (writing, drawing, etc.). He finds three "moments" in the rise of "tracery-imitation" (see his *résumé*, p. 587): A, an optical picture of the hand-movements (*optische Vorstellung der Handbewegung*) required for making the desired letter, derived from the child's earlier sight of his own hand-movements; B, a series of new motor discharges, strengthened by practice; C, a series of sensations of actual movement, by which the discharges are regulated and controlled. Moment A is clearly seen in the fact, often remarked, that in writing with the eyes closed we follow still a clear optical outline. In moment A there are two evident factors: (1), constant kinæsthetic memories (*Bilde*) from each position and each amount and direction of movement of the member, and (2) optical presentations of the same positions and movements (*optische intendirte Bewegungsbilde*). Moment C is for Goldschneider entirely kinæsthetic, as we would expect from his earlier papers.[1] He establishes, however, an entirely new element in this kinæsthetic complex in the case of writing, *i. e.*, a series of pressure sensations which vary with each character and each stroke. By a neat apparatus—consisting essentially of an air-cushion, upon which the pen-point of the subject rests, and a connected Marey tambour with graphic attachments—he demonstrates definite pressure curves for the different letters of the alphabet, punctuation marks, etc. [In the view of the present writer, this derivation of "tracery-imitation" is not adequate, simply for the reason that it assumes it. Given "moment" A—an association between definite hand-movements and the corresponding optical presentations of the same hand-movements—the latter might be sufficient, with practice, to innervate the former. But the question remains: How is this association established? How does the child come to connect the optical presentation of figure (*Gestalt*) with the optical hand-movement and kinæsthetic hand-movement series? G. does not recognize the fact that visual recognition of figure (pictures, letters, etc.) is definitely established long before the child is able or has any tendency to trace them.[2] He is wrong, accordingly, in identifying the optical figure series with the optical hand-movement series. An optical figure-series is really the first "moment" in the derivation of hand-writing — either a purely retinal series or an eye-movement series, according to the view adopted of the rise of visual perception of figure. The question then is: How does this optical figure-series come to stimulate the two muscular

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[1] See *Untersuchungen über den Muskelsinn*, *Du Bois-Reymond's Archiv*, 1889.
[2] See my article in *Science*, XIX, 1892, p. 16.

series (kinæsthetic hand-motor and ocular hand-motor)? My observations show that the process is as follows: As the child's experience widens, its optical perception of figures grows exact; certain retinal or eye-movement series grows more and more fixed. At this period the arm and hand-movement series, at first few and fixed, are broken up with the increasing mobility of the member. Consequently, from the arm-movement pictures those elements are emphasized, (1) which are seen as well as felt, and (2) those from the latter which produce results identical with elements in certain definite figure-series already established by the eye. This reproduction of visual figure-elements by movements which are seen establishes gradually the association between the kinæsthetic movement-images and visual figure-outlines; the ocular-movement elements (*optische Vorstellung der Handbewegung*) being, no doubt, an auxiliary, but not a necessary, middle term. In the figure,

$$\begin{array}{ccccccccc} & & a' & b' & c' & & & & \\ \times & - \times & - \times & - \times & - \times & - \times & - \times & \longrightarrow & Y \\ & & | & | & | & & & & \\ \times & - \times & - \times & - \times & - \times & - \times & - \times & \longrightarrow & X \\ & & a & b & c & & & & \end{array}$$

X= hand-movement-sensation series (*motorisches Bewegungsbild*), some of whose elements (a, b, c) at once reproduce (graphically) certain elements (a′, b′, c′) in the visual-figure series (Y= *optisches Bild des Gestaltes*). Now with the strengthening of this association by practice, appropriate arm (and hand) series follow promptly upon the eye (figure) series which constitutes the "copy." G.'s third "moment" is undoubtedly of great importance, whether or not it be entirely kinæsthetic.

The fact that the first moment is the visual-figure series, and not the visual-movement series, is seen (1) in the fact already cited that figure recognition arises first; (2) that we can trace figures, letters, etc., as G. says, with the foot, head, etc., by movements not usually seen; (3) the memories of the way the hand and pen look in writing are not at all clear compared with the two other elements, *i. e.*, the figure memory of the letters written, and the movement memories. For example, when I think of writing my own autograph, I picture clearly the figured letters which the point of my pen inscribes, and the muscular sensations of the hand, but hardly at all the way the hand looks in the successive stages of the signature; (4) in the analogous case of learning to repeat sound series, there are only two elements, the "copy" series—whether it be auditory (say the gutturals, which children sometimes learn first), visual, or speech-motor—and the resulting sound series (omitting G.'s third moment, the kinæsthetic control series as before); that is, there is nothing corresponding to G.'s optical-hand-movement series (*optische Bewegungsbilde*).]

Investigating, further, the "control" sensations—G.'s third moment—he finds, besides the pressure sensations mentioned above, that angular joint-movement sensations are most important. Beneath the threshold of perceptible joint-movement, pressure sensations still are felt, but pressure points are not distinguished. [This shows that some other control elements (perhaps central quantitative) are necessary for writing, executed by movements beneath the threshold of discrimination by joint sensations. Such writing has been demonstrated by Mohr.] By a further simple apparatus (see fig. p. 516), G. removes the resisting surface under writing movements, and finds that the curves of pressure for different strokes still remain, although the subject is not aware of giving different pressures.

On the pathological side, G. finds two classes of disturbances arising from impairment of the optical element and of the motor element. Under the former he cites left-handed writing, which is sometimes symmetrical with right-hand (*spiegelschrift*—from right to left), and sometimes a reproduction of the copy by the same movement as the right hand (from left to right). The former, "mirror-writing," he thinks is due to the tendency to symmetrical innervation of the two arms when one of them is practiced (*Mitbewegung*). It is possible only in as far as the optical "copy" image is thrown into the background or suppressed. The latter is due to the strength of this optical image in bringing both hands to the reproduction of itself. The motor disturbances of hand writing are also of two kinds, giving "trembling" (*Zitterschrift*) and "taxic" writing. The peripheral cause of the latter is impairment of any of the "control" series, visual, movement (joint), or pressure and resistance sensations. The central causes of disturbance he discusses in another paper (*Berliner klin. Wochenschrift*, 1892).

J. M. B.

Ein deutsches Gelehrtenleben von Prof. Dr. jur. J. E. Kuntze. GUSTAV THEODOR FECHNER. Leipzig, Breitkopf and Härtel, 1892, pp. 372.

The author of this volume is a nephew of Fechner's, and was for many years a member of his household.

Gustav Theodor Fechner was born in 1801 at Grossärchen, where his father had succeeded his grandfather as pastor of the church. His grandfather had won education and position by persistent struggle with poverty and discouragement, and his memory is still held in honor in the town where he lived as pastor over forty years. His father was remarkable for his independent and progressive spirit, which made him a leader among his people in matters of public utility as well as things spiritual. His mother was the daughter of a pastor, a woman of unusual force and sweetness of character. She was early left a widow in straitened circumstances with five little ones to bring up. The two boys were sent to an uncle, who provided for them for several years. In 1815, Frau Fechner was able to settle in Dresden, and the family was reunited for a short time. In 1817, Fechner entered the University of Leipzig to study medicine. He found most of the lectures he had planned to hear so unsatisfactory that he attended but two or three courses. Indeed, this dissatisfaction, combined with his distrust of the methods practiced in the profession, resulted in his giving it up altogether. He took the regular doctor's degree of the university, but devoted himself more and more to literary pursuits and scientific investigations. Even while studying in Dresden, he had partly met the expenses of his education by giving lessons and translating, and by these means he now earned sufficient for self-support. It was about this time (1820) that he read the first chapter of Oken's Naturphilosophie, which, to quote his own words, "so inspired me as to determine the direction of my mind for many years to come." To it he owed his escape from the atheism his medical studies had induced, and the permanent gain of the conception of all nature as a living unity.

In 1824 his mother and sisters joined him in Leipzig, and henceforth his home was with them until his marriage. He continued his translations from French scientific works, with voluminous additions of his own, and wrote besides many original papers on subjects connected with physics. To this period belong also his satires, which he published under the *nom de plume* of Dr. Mises. They are

marked by clear and admirable style, exuberant fancy and scathing wit, which spares neither philosophy, medicine, tradition nor convention. In the "Beweis dass der Mond aus Iodine besteht," as well as in some of the others, he gives humorous expression to the causes of his distrust of the methods then in vogue in medicine. In "Die vergleichende Anatomie der Engel" there are hints of conceptions later worked out more fully in "Nanna" and "Tend-Avesta." He began his lectures on physics at the university in 1824, and in 1834 received a full professorship. He had the most deep-seated distaste for any official restraint upon his intellectual life, and had only yielded to the solicitations of his friends in seeking the position to which he was finally appointed. A short time before he had married Fräulein Clara Volkmann, who proved a most faithful and devoted wife and companion, and who still survives him. The next six years were crowded with work, which Fechner felt obliged to pursue unremittingly, because of unavoidable family claims upon him for material aid. The crisis of his life was a natural result of this prolonged strain. In 1840 he gave signs of a complete nervous collapse, accompanied by severe head and eye troubles, which, for a time, completely disabled him. He has left a detailed account of this illness, believing that it may be of interest and service to others. The beginning of his gradual restoration to health of mind and body reads like the story of some miracle. Slowly but surely the improvement went on, and about 1843 he was once more able to resume his accustomed life and work with the joy and vigor of one risen from the dead.

During his illness a pension had been assigned him by the university, and although he never again assumed his former position, he gave voluntary lectures on the relations between mind and body for many years. He was not popular as a lecturer, except with a chosen few, for the average student craved instruction given on more limited lines than those on which his mind naturally worked.

Fechner's illness made the one serious interruption to his lifelong intellectual activity. After his recovery he was able to devote himself to philosophic and scientific research and production for more than forty years. To his period belong his best known and most important works. It seems appropriate that the detailed exposition of his system of philosophy should be among the first fruits of this time. In 1848 "Nanna" appeared, and in 1851, "Tend-Avesta." In both of these he urges with eloquence and force the ground for his belief in that higher pantheism which was his deepest personal conviction. In nothing that he has written does he give greater evidence of his freshness and originality as a thinker, as well as of his rare literary gifts. His best-known contribution to science, the great work on "Psychophysik," appeared in 1860, and proved him a master in the new field, which had been but recently opened up. Wundt says "Fechner was the first to recognize the extent and significance of the investigations of Weber, and to him is due the creation of those exact methods without which further progress would have been impossible." The period of Fechner's interest and work in æsthetics, which now followed, is described by his biographer as a parenthesis. He threw himself into the subject with that ardor which characterized him in everything he undertook. His "Vorschule der Æsthetik" was the final exposition of his views, and in it he establishes a scientific basis for some of the fundamental ideas of beauty. The labor of his last years was chiefly given to the revision and strengthening of his work in "Psychophysik." Old age found him with unabated mental freshness and vigor. He was at work until his usual hour the evening that he

was taken ill. He lingered a few days, unconscious for the most part, but whenever he revived, his mind was perfectly clear. He fell quietly asleep, Nov. 18. 1887.

This life shows us a man endowed with a rare combination of qualities, both moral and intellectual. Simple and kindly as a child, indifferent to luxury—his one extravagance was in the matter of writing material—brave and patient in suffering, and afraid of nothing but idleness, of genuine piety and purity of life, he might almost have passed for a saint had he not been so remarkable as a scholar. Possessed of unusual independence, originality and versatility of mind, he yet had infinite patience in mastering subjects in themselves distasteful to him. Great as was his productiveness he did not write easily, and all his work represents painstaking revision and correction. Modestly conscious of his own powers, it is not strange that he was disappointed at the comparatively small recognition with which his philosophical system had met. Content with no subject until he had brought it into some sort of unity with his conception of the whole, he could ill understand the ability of many men to hold confused and inconsequent views. He was a true son of his people in his passion for nature, and his deep sense of man's close and intimate relaion to it. But he had his limitations. He was distinctly the nature-lover as distinguished from the lover of history and the past. Rome

> "A city was to him
> And it was nothing more,"

and he walked its streets far more interested in his own thoughts than anything outside them. For general social life and amusement he cared not at all. His one regular diversion was a weekly chess club, whose meeting he never missed, and he always enjoyed the society of intimate and congenial friends, and was often stimulated by them to discussion and argument, in which he took a keen delight. It is still too early to assign him his final place in the intellectual history of his country, but he is a connecting link between the past and present, and represents that scientific insterest in the physical side of mental phenomena which marks a new era in German philosophy. His great aim was to bring about the alliance of speculation and research, and in this good cause he fought loyally to the end.

C. H. S.

Ueber die Gleichzeitigkeit und Ungleichzeitigkeit von Bewegungen." O. KÜLPE. Phil. Studien, 1891, VI, p. 514, and 1892, VII, p. 124.

Dr. Külpe discusses some experiments in which it was sought to raise both hands simultaneously. The subjects reached in four ways: ordinary muscular and sensorial reaction, and two unstimulated kinds, one with attention previously concentrated on the movement, as in muscular reaction, and one without special preparation. A bell furnished the stimulus (where necessary) in the first set; only such results were admitted as appeared simultaneous to the subjects. The figures given represent the deviations from simultaneity. In the majority of cases the left hand reacted first. The several subjects and kinds of reaction give widely varying results, but in general the figures for muscular and sensorial show the same relations as in simple reaction, and so do the unstimulated reaction —the average of the so-called "premeditated voluntary reaction" being slightly larger than that of the muscular, and that of the "unpremeditated" rather larger than that of the sensorial.

Looking at reaction from the psychological side, two laws of the association affect its rapidity. First, a period of strained attention

gives rise to a displeasurable feeling, which hastens transition. Second, the more closely states are related the quicker the transition. The coördinations or reaction are not natural, but in muscular reaction the *idea* of the coördination forms a natural connecting link. As to the former factor, in muscular reaction the attention is strained, hence the passage to movement is more rapid than in sensorial. In reacting with two hands, the deviation from simultaneity may be taken absolutely, or distinguished by plus and minus signs (*i. e.*, relatively).

To determine the causes for the priority of one hand, further experiments were made. First, with other kinds of stimulus: a hammer, light, and electrical stimulation of the skin. This produced no noticeable change of result. Next, the fingers of one or both hands were made anesthetic by ether or ice, thus removing the sensation of touching the reaction-key. This lowered the mean variation perceptibly, without altering the differences. In other experiments the efficiency of the muscles on both sides was diminished by a strong electric current; this increased decidedly the average priority of the left hand. To test the influence of *attention*, one hand was consciously singled out and attention directed more closely to its movement. Comparing these results with the others, there is a marked increase of preference for the left, when that hand is designated, and generally (but less noticeable) for the right, when it is designated. In some final experiments the muscles of one arm were fatigued by tension; the effect was to *delay* the action of that hand, especially in case of the right.

Dr. Külpe argues that, since the right hand is more accustomed to grasping and pressing than the left, it would usually press harder on the reaction-key, and (greater fatigue ensuing) would react more slowly than the left. But the left hand is more dependent; hence special attention to the right benefits it less than attention to the left. Chance directing the attention may therefore explain the variations in the earlier experiments. The larger variations in sensorial reaction may be because the preconceived idea of the movement represents only the transition from apperception to movement, while in muscular it includes the entire preparation for the movement, leaving less open to chance variation. In anesthesia the idea of the coming movement is not in conflict with the sensation of the present position of the hand. With the latter present, more or less of a counter-effort is required to prevent the movement from immediate accomplishment, and this increases the attention and fatigue.

The influence of this conflict between expectation and tension on the course and duration of reaction is to be investigated in another paper. HOWARD C. WARREN.

The subliminal consciousness. MEYERS. Proceedings of the Society for Psychical Research, 1892, Feb. 1.

Holding that automatic writing, trans-utterance, automatic picture-drawing, crystal vision, monitory voices, hallucinations and kindred phenomena are in no sense abnormal, the author proposes a hypothesis for "the provisional coördination of all these subliminal phenomena," which "does not need constant stretching to meet the exigencies of each fresh case." Assuming that we must be in some sense conscious of any sensation or volition which we can afterwards recall, it is evident that multitudes of things have entered into consciousness without our knowledge. "Our habitual or empirical consciousness" is a selection of such parts of the whole

as have proved to be, in general, advantageous to the individual. It is related to the whole of consciousness much as the visible part is to the whole of the solar spectrum. The underlying psychical unit, the abiding "individuality" in each of us, is expressed, but always incompletely, as the "personality" of our ordinary waking state. At the lower, or physiological, end are processes that have become automatic; at the superior, or psychical, end are clairvoyant and other impressions which are habitually received, and which "do in some sense transcend the limitations, of time as well as space, within which all supraliminal consciousness necessarily falls."

Examples of changed personality suggest the possibility of such a psychical reorganization as shall incorporate into our ordinary personality powers now entirely subliminal, and impressions which now reach us occasionally as "messages" from the subliminal part of our individuality, which become visual or auditory or indefinite according to the character of the personality and the attending circumstances. Such "messages" may produce hallucinations which are objective in the sense that their source is not in the individual.

Dreams are a familiar example of slight changes of personality. Hypnotic trance, hysteria and insanity are other examples. The author adduces evidence in favor of his view that "subliminal consciousness" is entitled to the epithet "conscious;" and compares the contents of supernormal phenomena with what would be expected from his hypothesis. T. P. HALL.

Aufäuge und Aussichten der experimentellen Psychologie. KÜLPE Archiv für Geschichte der Psychologie, Bd. VI., Heft 2.

An historical outline. Contributions to *experimental* psychology have been made all along by physics and physiology; but the real question is, Why was psychology so slow in becoming an *independent* science? Because, first, of the neglect with which the "lower faculties" were treated; and again, because of Kant's unfavorable verdict, called forth by the empirical psychology of the 18th century. His objections were met partly by Herbart's mathematical psychology, partly by the actual founding of experimental psychology. This owes its existence, after Weber's suggestive work, to Fechner, who by demonstrating the functional relations between psychical and physical processes, did away with the inexactness of earlier psychologies, and by developing psycho-physical methods, supplied the necessary means of research. If his work is in some respects imperfect, and his estimate of Weber's law too high, it must be remembered, on the other hand, how scant was the material gotten up by his predecessor.

Wundt, the next leader, though at first under the influence of "pure" psychology, develops to the fullest, in his later work, the principle of parallelism. For him the correlative of psychical activity is the nerve-process, while for Fechner it is the outer stimulus. The advances made by these men within the domain of psychology proper, have been seconded by work in other branches, especially astronomy and physiology. As to pathology and zoölogy, important as their results may be, they offer no room for what is, strictly speaking, psychological experiment. E. PACE.

THE AMERICAN

JOURNAL OF PSYCHOLOGY

Edited by G. STANLEY HALL.

VOL. V. JULY, 1893. NO. 4.

SOME PRACTICAL SUGGESTIONS ON THE EQUIPMENT OF A PSYCHOLOGICAL LABORATORY.

BY EDMUND C. SANFORD, PH. D.

The kind of equipment a psychological laboratory is to have should be controlled by the needs of the students that are to use it, the amount of money at command, and the special lines of interest of the instructor in charge. To give detailed advice without detailed information on these points is impossible. It is hoped, however, that a few general suggestions with regard to rooms, apparatus and method of instruction, though without novelty to those already in possession of laboratories, may not come amiss to those having them in contemplation.

ROOMS.

This important part of the laboratory is unfortunately too often not under the control of those most interested. The laboratory must occupy such rooms as are free for it. As a younger member in the family of sciences, psychology must be content with the outgrown clothes of its elders. If any choice is possible several points should be regarded, and first of all, quiet. It is relatively easy to shield the eyes and skin from intrusive stimulation, but it is extremely difficult to shield the ears; and what freedom from jar is to the physicist, that freedom from noise is to the experimental psychologist. Heating, lighting and ventilation are important in all study rooms, and *a fortiori* in rooms where bodily conditions must

be kept constant and prevented as far as possible from disturbing mental conditions.

As to size, a number of small rooms are better than an equal floor space thrown into one or two large rooms, for there are not many psychological experiments that can be made simultaneously in the same room without mutual interference, and for the few that require considerable distances, it is better to depend on a spare lecture room or chapel. The most convenient arrangement is one large room for a general laboratory and apparatus room, and several small ones that can be used, though not exclusively, for special purposes: one for the instructor's private laboratory, one for light and color experiments, one for time experiments, a storeroom for bulky apparatus, and especially one for a work-shop, though this need not be adjacent to the rest. It is also convenient to have a small room or closet, suitably connected by wire with the other rooms, in which all batteries may be kept and cared for. There is a temptation to use the large laboratory room as a lecture room also, but this should be resisted on account of the dust from the blackboard.

The situation of the rooms is relatively unimportant, except in the case of rooms where light and color experiments are to be made, which should have a southerly exposure, so that direct sunlight may be had. Proximity to highly colored buildings or green trees is to be considered, and there should be no tinting on the walls. Rooms in an upper story of a large building have an advantage both as to light and stillness.[1]

If it is inexpedient to devote a room exclusively to light and color, *i. e.*, to make a dark room of it, much can be done with dark boxes of convenient size; or, following the plan of the Yale laboratory, the dark box may be enlarged till it will take in the experimenter himself and becomes a room within a room. Such an arrangement has much to recommend it.

FURNITURE.

All the rooms should have gas or electric lights for illumination; the general laboratory and the work-shop should have gas for heating; and the first should have a sink and water. The general laboratory and the dark room should be provided with means for excluding light. For the first, where only a relative darkening is needed, black curtains or curtains of enameled cloth will answer, but an extra casing must be fastened to the window frame, covering the edges of

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[1] See plan of the Toronto laboratory where these points are regarded, *Science*, XIX, 1892, 143.

the curtains and preventing the light from getting in beside them. The curtains must pull up from the bottom, not down from the top. More pains will have to be taken with the dark room, and there solid shutters of some kind, painted black like the walls of the room, will probably be as satisfactory as anything.[1]

Tables for laboratory use should not be so good as to be marred by an occasional tack driven into them. Some should be large (3x8 ft. or longer) and some small (2x4 ft.). If some are thirty-six inches high (for use when the experimenter stands), and others are six inches less (for use when he is seated), it will be well; and it will not be amiss if all are provided with drawers. It is convenient to have one or two very solid tables with square legs the same size at the top and the bottom, so that apparatus may be clamped to them. There should also be a few small tables that are adjustable in height. They are almost indispensable when several irregular pieces of apparatus are to be brought to the same level for combined use. Such tables with iron standards can be bought, or they may be made wholly of wood by any carpenter. A wedge to hold the table at the required height is better than a screw, for it does not mar the stem of the table and is more effective against wabbling. Three sizes at least are handy: a small size to stand on the ordinary tables and capable of adjustment from twelve inches to eighteen inches in height; a larger size capable of adjustment from twenty-four to thirty-six inches; and a third size adjustable from four to six feet. For a purpose similar to that of these tables the laboratory should have a good supply of smooth blocks, six or eight inches square and from one to two inches thick; also a few smooth bricks, which may be permanently covered with thick paper for greater cleanliness in use.

A shelf running along the side of the room and before the windows at about the height of the ordinary tables, is very convenient. If it seems undesirable to fasten such a shelf permanently, narrow tables or benches may well take its place. Chairs for the laboratory may be of any comfortable sort; physical discomfort is a serious hindrance to successful work for both the subject and the operator. There must be also a few screw stools, so that the height of the observer with reference to his instrument may be readily adjusted.

The number and character of the apparatus cases will be fixed by the apparatus to be placed in them, but they should be large enough to prevent crowding and some of them at

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[1] Aubert gives some particulars about the construction of a dark room in his *Physiologie der Netzhaut*, pp. 26 ff.

least should be closed with hinged glass doors; sliding doors are apt to strike and break apparatus carelessly put in.

Either below the apparatus cases or in a case by themselves, the laboratory must have a plentiful supply of drawers, and some of these should be divided by low partitions for the more easy keeping of such things as kymograph papers, small diagrams for optical experiments, etc. Drawers that are to contain many kinds of things, as, for example, the drawer for nails, screws and tacks, may conveniently be fitted with a number of little compartmented trays instead of fixed partitions. The trays can then be taken out and carried about with their contents as needed and again returned to their place in the drawer. Some of the drawers should be large enough to allow full sheets of cardboard to lie flat in them, unless they can be kept in a chart case.

In the matter of chart cases there is great diversity of practice. A convenient way where one has control of the making of his charts is to have them all drawn upon uniform sheets of manilla or other cardboard (using several separate sheets for large diagrams), and then have a case of shallow drawers in which to keep them. These drawers should have backs and sides, but no fronts, or fronts that are hinged and can be turned forward out of the way. In such a case the charts are kept flat and the edges of all can be easily examined without disturbing any. The front of the case can be protected by doors or a curtain.

Many colored papers fade if long exposed to the light and must be protected from it. A convenient way to do this with disks already cut for use on the color-mixer, and yet to have them easy of access, is to prepare a special case for them. A piece of pine plank two inches thick, somewhat wider and four or five inches longer than the diameter of the disks, has near one end a circular hole cut through it just large enough to take in the disks. From the side of this hole next the longer end, a slot an inch and half wide is cut nearly or quite to the end. A thin piece of board of the same size as the plank is nailed on for a bottom; a similar piece is hinged on the top for a lid, and a narrow piece nailed across the end of the slot, if it has been cut entirely through, and the case is complete. When the colored disks are laid in, the different colors are kept separate by disks of ordinary cardboard. These have tongues that lie in the above mentioned slot (the longest ones at the bottom), each bearing the name of the color that has been put in below it. Any required color can then be found at once by lifting the tongue bearing its name.

In the general laboratory there should be a bookshelf containing the textbooks most frequently consulted, including a stand-

ard text-book of physics and a book of mathematical tables. And near by may well be kept a card catalogue of psychological literature. Such a bibliography, if contributed to from the reading of all users of the laboratory, would soon grow into a most valuable aid to research. In the cabinet may also be kept a card catalogue of the apparatus, giving the name of the maker, date of receipt and price of each piece, and in addition any constants or corrections that it may be necessary to know for the accurate use of the piece in question. This list will be found useful, not only in checking up apparatus at times of stock-taking, but also in giving students items about the manufacture of the apparatus that they may be interested to know.

Apparatus.

If a carpenter and skilled machinist are at command, comparatively few pieces of apparatus will need to be bought outright, and much of the rest will be cheaper made at home. Even when such help is not forthcoming, the instructor himself, if moderately familiar with the rudiments of wood and metal work, can do a good deal. A certain knowledge of these arts is important, even for the successful use of boughten apparatus, and every instructor should take pains to acquire it. Such hand work will often be found an agreeable change from book work.

Assuming that the instructor has a little mechanical skill, and that professional mechanics will only be appealed to in cases of especial difficulty, the first room of the laboratory to be fitted up may well be the work shop. Here should be found a small bench with both carpenter's and machinist's vises, and the most common tools for wood and metal work. A lathe also should certainly be added, with a sufficient stock of chucks and lathe tools, though an excessively high-priced machine is not necessary. A hundred dollars expended upon tools of all sorts and fifty dollars more upon the fitting up of the room and the purchase of materials, would probably be sufficient, and would soon save its value in the making and repair of the strictly psychological equipment. If nothing of this sort is possible, a few tools at any rate are indispensable; large and small screw-drivers, a wrench, a hammer, with nails and tacks, a meter stick, and an oil-can are perhaps the minimum collection.

The strictly psychological apparatus to be purchased will vary with the plan of work and with the facilities for borrowing from the physical and biological departments. If, as is probably the case in most American colleges, demonstrations

are to be made before a class of twenty or upward, a practice laboratory course given to a less number, and research to be carried on by the instructor and one or two advanced students, and if the department is expected to stand on its own feet without much borrowing, the stock of apparatus may be somewhat as follows :

Apparatus for neurological demonstrations. Models of the brain[1] and sense organs; microscopes and mounted specimens; frogs; sheep's brains and facilities for removing and preserving them;[2] reagents; diagrams; to a total of about three hundred and fifty dollars.

Apparatus for the senses.[3] Since a good part of the work so far accomplished in physiological psychology has been upon sensation and perception, this section of apparatus will naturally be pretty full, especially as many pieces are also of use for the study of the higher forms of mental life. For the senses of taste and smell a very small expenditure is sufficient. For the dermal senses and sensations of motion the apparatus, except for advanced research, is simple, and much of it can be made by any carpenter. An allowance of one hundred and fifty dollars should cover everything. For auditory experiments more refined apparatus is required. It would be easy here to spend two hundred and fifty dollars without exceeding the bounds of economy. Apparatus for vision and the visual perception of space, including in this a good supply of stereoscopic and other diagrams, would require perhaps three hundred dollars. Some allowance should also be made for apparatus for the study of pain, a promising subject as yet little investigated, making a total for all the senses of something over seven hundred dollars.

Time apparatus. A very successful means of study of the higher mental functions has been the measuring of their time relations. The standard instrument for this is the Hipp chronoscope, which itself costs about seventy dollars, and requires for full usefulness batteries, testing apparatus, electric keys, commutators, etc., to the amount of perhaps a hundred dollars more. Of almost equal importance and of more varied usefulness is the Ludwig kymograph, an apparatus for furnishing uniform motion, either to the drum that forms part of the apparatus itself, or, as a motor, to other light pieces

[1] See Notes on Models of the Brain, by H. H. Donaldson, AMER. JOUR. PSYCHOL. IV. 130.

[2] See chapter on the Structure of the Brain in James's briefer course on Psychology.

[3] For detailed suggestion as to apparatus for the senses, I may refer to the introductions to the successive sections of my laboratory course in earlier numbers of this JOURNAL.

of apparatus. The instrument is delicate, and is expensive (it costs about two hundred dollars), but in very many lines of work its absence is a great loss. There are, however, a number of cheaper substitutes for it which could probably be made to answer most purposes, and even if a kymograph is included, it will be best to include also some simpler form of rotating drum to use when regularity of motion is not required. As in the case of the chronoscope, a good deal of accessory apparatus is required to get the full advantage of this central piece; there should be two or more electrical time-markers, an electrically excited vibrator and a tuning fork of 100 v. d. per sec., Marey tambours, etc., etc., with conveniences for smoking the paper-covered drums and fixing the tracings when made. For the chronoscope and kymograph and their appurtenances, an allowance of at least five hundred dollars should be made.

The psychophysic law may be studied with apparatus already included under the foregoing heads, but some new pieces or adaptations of those already mentioned are useful, and for them seventy-five dollars may be set aside.

General apparatus. In addition to the apparatus for more or less specific uses there is a class of general apparatus that is no less important; and chief among this class is a good-sized and substantial collection of stands, rods and clamps. Money spent on these will be well invested, as the later saving of time and exasperation will demonstrate.[1] There should also be included a number of black, white and gray screens that may be made to fit the rods and stands. A plethysmograph and sphygmograph shonld be in the collection. There should be some kind of a motor in the laboratory more powerful than the kymograph, electric or water, as convenient. A good set of drawing instruments, with brushes and colors, will find frequent use; also a number of beakers and flasks, several graduates and a pair of scales with weights. There should be a clock, towels, wastebaskets and slop jars, dust-pan and brush, several china plates, cloth for covering apparatus outside of the cases when not in use, and a good deal of other miscellaneous stuff, which, with the rods and clamps and the rest, may consume as much as two hundred dollars. In the laboratory should also be found a moderate stock of cardboard (black and white), colored papers, pins, needles and thread, mucilage, glass tubes and rods, alcohol, mercury, shellac varnish, sealing-wax, corks (rubber and ordinary), rubber tubing and sheets, hard rubber, shot, sheet lead, cotton batting, etc., etc., which might be covered by twenty-five or thirty dollars.

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[1] On apparatus of this sort see AMER. JOUR. PSY. V. 476, 499.

The total cost of apparatus on the scale indicated, without allowing for the duplication that might be needed for large classes, is thus something above two thousand dollars, but no allowance has been made for transportation of imported apparatus (a large item), nor for such special pieces as would be desired for special original researches, nor yet for the fitting and furnishing the rooms. It is not an overestimate to say that a fully-equipped laboratory in an institution of college grade may be expected to cost between four and five thousand dollars. A sum from one hundred to two hundred and fifty dollars a year would be needed for supplies, repairs and the purchase of new apparatus, and these figures would have to be still larger if many students were engaged in research.

A beginning, however, can be made with a good deal less than five thousand dollars. In the hands of a mechanically skillful instructor a tenth of that sum spent upon tools and cardinal pieces of apparatus, though wasteful of the instructor's time, would give a very fair start, and even two hundred and fifty dollars spent on apparatus alone would do much to enliven and fructify the course in psychology. If a starvation appropriation is all that is to be had, the most satisfactory pieces would probably be: a sonometer and a few tuning-forks for audition, a color-mixer and Wheatstone stereoscope for vision (the latter home made), and a stop watch for time measurements.

ON THE USE OF THE LABORATORY.

Psychological experiments fall roughly into three classes: first, those that can easily be made by a large number of persons at once, as some experiments on hearing and vision, and also some on association and attention; second, those that can be made quickly and easily, but by only one person at a time without duplication of apparatus, including many experiments on touch, subjective visual and auditory phenomena, and binocular vision; and third, experiments that require a considerable time, including all the quantitative experiments, reaction-times, memory and memory span, psycho-physic law, etc., where the average of a number of individual tests is necessary to give a sure result. This difference will of course be recognized in planning the work of the department. It would be a great blunder to rob the lecture course of its illustrative experiments to crowd them into the laboratory. Experiments of the second class may well go into a demonstration hour in the general laboratory room following the lecture, when, without formality, apparatus may be passed from hand to hand and questions asked and explanations

given. To these also should be added some of the experiments of the third class, given in a demonstrational way, for the sake of students who do not follow the subject further. The third class of experiments in their rigor should be reserved for attack at another hour with those who wish serious laboratory work.

Most teachers, I believe, will find it difficult, at least in the present state of experimental psychological courses, to keep more than six or eight students profitably busy at the same time, especially at first, when some may have a merely spectacular interest in the subject. Larger numbers must be handled in sections.

In many psychological experiments it is necessary that two persons work together, one as subject and one as operator, and for this purpose a selection of partners for the course at the beginning is to be recommended. The order in which work is taken up in the laboratory is not of extreme importance, and if the apparatus contains no duplicates, as many lines of work must be started at once as there are partnerships of students. The following half dozen lines of work will illustrate what I mean, though every instructor will probably prefer to frame his own: 1. The senses, experiments not included in the lecture and demonstrational courses. 2. Reflex action and selected nerve-muscle experiments. 3. Reaction-times and related experiments. 4. Memory span for sounds, letters and numbers, and with distracted attention. Card sorting test for memory as described by Bergström in the last number of this JOURNAL, and possibly some adaptation of a few of Ebbinghaus's experiments with nonsense syllables. 5. Attention and its motor accompaniments, inversion of time order of sensations, as shown in Exner's and Dvoràk's experiments. 6. Weber's law, with detailed work on some one method, with full demonstrations of the others.

In the prosecution of these and all other experiments, the art of the teacher will appear in leading the students to observe for themselves and to draw their own inferences. They should at the same time be shown how to keep intelligible records of their work. Indeed all the pedagogical principles already established for laboratory physics, chemistry and biology apply with equal force here, and very much of primary importance may be learned from experienced teachers of these subjects, both with reference to the furnishing of the laboratory and the handling of the pupils in it.

After so much of a general orientation as would be given by work in several of these lines, the student may enter upon original work as an apprentice to the instructor, serving alternately as subject and operator, and being

encouraged to contribute in every way to the success of the research and to feel a part of the responsibility for its scientific character.

At some place in the course, room should be found for the gathering among the class or the college at large of statistics of colored-hearing, number forms, lists of associated words, etc., not only as a means of interesting the pupils, but also as a means of giving them, in the proper working up of the figures, some training in the handling of statistics and some insight into the large fields of observational psychology in which the statistical method seems at present our chief resource.[1] To this, if opportunity offers, may well be added the observation of the behavior of certain lower forms of life and of the domestic annimals, especially when young, and of children.

It is hardly necessary to say in conclusion that any one having the equipment of a laboratory in mind should not fail, even at some expense of time and money, to visit as many as possible of the existing laboratories and learn by direct inspection and conversation what his colleagues have found desirable and what is to be avoided.

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[1] For accounts of such studies, though executed by instructors instead of pupils, see Jastrow, *Educational Review*, II. 1891, 442–452; Patrick, *Popular Science Monthly*, Feb., 1893; and the paper of Mary Whiton Calkins in this number of the JOURNAL OF PSYCHOLOGY.

A STATISTICAL STUDY OF PSEUDO-CHROMESTHESIA AND OF MENTAL-FORMS.

By Mary Whiton Calkins.

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In the spring of 1892, the Wellesley College class in experimental psychology began a study of cases of pseudo-chromesthesia and of mental "forms." A canvass of the college was undertaken and records of outside cases were also collected.[1] Since that time a few new records have been added and a very detailed investigation of all cases has been made, on the basis of a series of questions which were formulated after the careful study of the first records. The results are summarized here with the briefest possible comment.

In the first table, reference is made only to the records from members of Wellesley College; and so large a number of persons have been consulted that the per cents may perhaps fairly be supposed to suggest the common prevalence of the phenomenon.[2]

[1] A short account of this work was given in the American Journal of Psychology, Vol. V., No. 2. Variations of the present record from that are due chiefly to the new material collected; but occasionally to later, more detailed statements of the same subjects. The figures given, throughout this article, have been carefully verified, and every effort has been made to interpret accurately, through personal interviews, or through correspondence, the exact meaning of the subjects. Hundreds of letters have been written and scores of interviews have been held.

[2] Since the completion of this paper, a canvass has been made of the students who entered Wellesley in the fall of 1892. Its results differ remarkably from those of Summary I., in the far larger proportion of cases, both of pseudo-chromesthesia and of forms.

Of 203 consulted, the number of persons with pseudo-chromesthesia is	32 (=15.7%)
Number of persons with forms is	61 (=30.2%)
Number of persons with *both* is	17 (= 8.4%)

The result may be accidental, but it is possible, on the other hand, that among the two hundred or more, last year, whom our questions did not reach, was a relatively large proportion of subjects. It is proposed to attempt a mediation between the two results, by continuing this canvass with successive freshman classes.

Summary I.

Comparative Frequency of Pseudo-chromesthesia and of Forms.

Total number of persons consulted,	525	
Number of persons with pseudo-chromesthesia,	35	(=6.66%)
Number of persons with forms,	65	(=12.38%)
(Note: Number of persons with *both*,	18=3.42%)	

The other summaries deal with all recorded cases, including those outside of Wellesley. The first of these attempts a sub-classification of

Summary II.

Varieties of Forms. (Total Number of Subjects, 85.[1])

	Verified.	Constant.	Unverified.	Total.
Month-forms.	55	13	5	73
Number-forms.	50	10	7	67
Day-of-week-forms.	23	23	4	50
Century-forms.	4	7	1	12
Hour-forms.	3	1	1	5
Alphabet-forms.	6	37	2	45
Totals.	141	91	20	252

In the first column of this summary are included cases in which the forms have been drawn in the same way after an interval (in most cases a year, occasionally only a few months) following the first record. A few cases are counted in which the second form varies slightly from the first (for instance, bends at the same number and at the same angle, but to the left rather than to the right). The second column contains the record of forms which did not appear on the first record, where the omission was a mere neglect and the subject explicitly testifies to the constant possession of the forms. The third column includes not only the unverified cases, but also those in which the second drawing of the form differs from the first.

Among the hour-forms and the century-forms are included only the cases in which these are unlike the number-forms; to most subjects the number-form is used for both series. The alphabet-forms are probably, most of them, mere visual

[1] Evidently the record of the same subject appears often under several of these heads.

reproductions of the primer page, so that they hardly belong to our summary.[1]

A few curious forms are not included at all. Among these are forms for piano notes (squares), with lines for violin notes; and an interesting prayer-form, well remembered from the time when the progress from one prayer to another was always the passage from one part into another of the form.[2]

The next table is a classification of

SUMMARY III.

Varieties of Pseudo-chromesthesia. (Total Number of Subjects, 45.[3])

Color with letters:			
With consonants only,		5[4]	
With vowels only,		4[4]	
With both vowels and consonants,		20	
			29 cases.
Color with the numerals:			10 cases.
Color with words:			
With common and proper terms,		20[5]	
With common terms only,		1	
With proper names only,			
Names of people only,	7		
Names of days only,	1		
Names of months and days,	1		
Names of people, months and days,	8		
		17	
			38 cases.
Color with music:			
With notes of different pitch,		8	
With different pitch and different instruments,		3	
With different composers or compositions,		4	
With other varieties and combinations,		8	
			23 cases.
Total varieties of pseudo-chromesthesia,			100[3] cases.

The connection of particular colors with the different letters is a widely varying one. All the associations seem fortuitous, except possibly that of *i* with black and of *o* with white. Dr. Jordan[6] explains this as certainly due to the

[1] But there are some distinctive alphabet-forms. Cf. Fig. 3, Plate I.

[2] See also Fig. 7, Plate I.

[3] Evidently the record of the same subject often appears under several of these heads.

This contradiction of Galton's generalization has already been noticed. AMERICAN JOURNAL OF PSYCHOLOGY, Vol. V., No. 2.

[5] There are four cases of color with *all* words.

Cf. D. S. Jordan, *Pop. Sci. Mo.* XXXIX. 67.

appearance of the letters ; but, in the case of *o*, an explanation given in one of my records seems equally plausible, and is interesting because the subject is now blind.[1] "*o*," she says, "=cipher=blank=sheet of white paper."

SUMMARY IV.

Colors with Different Letters.

I. With i:		
i is black in		11 cases.
i is "nearly black" in		4 "
i is grey in		3 "
i is "white" or "light" in		2 "
i is cardinal in		1 case.
Total cases of color-associations with *i*,		21
II. With o:		
o is white in		11 cases.
o is "greyish-white" in	3	
o is "bluish-white" in	1	
o is "colorless or white" in	1	nearly white in 7 "
o is "white or yellow" in	1	
o is "light" in	1	
o is grey in		1 case.
o is "golden-brown" in		1 "
o is black or "very dark" in		2 cases.
Total cases of color-associations with o,		22
III. With a:		
a is blue in		7 cases.
a is red in		4 "
a is of other color in		11 "
Total cases of color-association with *a*,		22
IV. With e:		
e is yellow in		8 cases.
e is of other color in		15 "
Total cases of color-associations with *e*,		23
V. With s:		
s is yellow in		6 cases.
s is red in		3 "
s is "red or yellow" in		1 case.
s is "yellowish-red" and "reddish-yellow" in		2 cases.
s is of other color (blue 4 times) in		10 "
Total cases of color-association with *s*,		22

The relative frequency of the connection of the color with the sound or with the appearance of letter or word has been carefully studied. Of course in the cases of merely musical association—except in the few of color with the printed notes

[1] One of the two records from students of Perkins Institute.

—the color follows the sound; and probably this is also true where merely the vowels or merely the consonants have color, since the distinction here is one of sound, not of appearance. On the other hand, in cases of association with numerals (as distinguished from the names of the numbers), the color follows the appearance. In the remaining varieties, we find the association of color with sound most frequent; so that the name "colored-hearing" is partly justified.

SUMMARY V.

Connection of Color with Sound and with Appearance.

a. Color when Letter or Word is						*b.* Color when Letter or Word is Imagined as						
Heard (only).	Seen only.	Both.	Total.	Unv.	Total. Cases.	Heard.	Seen.	Both.	Total.	Never.	Unv.	Total. Cases.
14	1	28	43	2	45	11	1	27	39	4	2	45

To discover exactly the manner in which the color appears to a subject is very difficult. To some the color is so indefinite that it is almost impossible for them to describe it; but to a larger number, the experience is so clear that they assume its universality and can hardly be prevailed upon to describe it in detail. "It was not recorded last year," one subject says of her month-form, "because I did not realize that so simple an arrangement could be a 'form.'" Great care has, therefore, been exercised to make these results correct interpretations of actual experience.

In the following summary, column (*a*) includes cases in which the color appears as a background to letter or to word; (*b*) those in which each letter is colored (as if printed in colored ink); (*c*) those in which the letters are colored, but of one color; (*d*) that larger number of instances in which the color appears in more or less vague form—not that of the letters—either after or with word, music[1] or letters: evidently this class contains all cases of musical color-association.

[1]In one of this year's records the music-color is thus described: "I imagine a prismatic band somewhere around the keys (I can't decide whether it is over or under them)."

SUMMARY VI.

Manner of Word-color.

(a)	(b)	(c)	(d)	(e)	Total.	Unv.	Total Cases.
1	4	9	19	3	36	2	38

The connection in cases of letter and word-association between the coloring of words and that of their letters, is peculiarly baffling. It is impossible to reduce it to rule; often, in spite of definite associations of color with the letters, the word has a color different from that of any of the letters composing it. Very often such a word, with independent color of its own, may be made to assume the color of its various letters by mentally fixating each of these; but this is ordinarily at the expense of any appreciation of the word as a whole. In the next summary, numbers under (*a*) refer to cases in which the word follows the color of its initial letter; under (*b*) and (*c*) to those in which the color seems to be that of a predominating vowel or letter; under (*d*) to those in which each letter retains its color. The class of associations in which no connection is traced between letter and word-coloring is marked by (*e*). It will be observed that, followed closely by this class of the undetermined, the one in which word-coloring follows the initial is best filled. Since the word-colors of the same subject may be determined at different times by different principles, the same "case" may be recorded under more than one of the headings. These double records are indicated in the table by a (?), which is meant to suggest that few words of the subject belong to the given category. No cases are twice counted in the totals.

SUMMARY VII.

Connection of Word-color with Letter-color.

(a)	(b)	(c)	(d)	(e)	Total.	No Letter-color.	Total.
? 9[4]	? 5[2]	? 4[2]	? 1[3]	? 8[5]	27	11	38

The value of our records, in explaining the phenomenon which they describe, must be admitted at the outset to be suggestive rather than demonstrative. Of possible theories there are, of course, two, which may be roughly characterized as the psychical and the physiological. The first refers

pseudo-chromesthesia and mental forms to ordinary associations, probably of childhood. The second finds no explanation, except in an assumed cerebral peculiarity—an especially close connection between certain brain tracts, especially the visual and the auditory. This assumption of the existence of apparently inexplicable brain peculiarities is, however, little more than a confession of ignorance; and the psychical theory will evidently be preferred, if it can be substantiated. But it is difficult to draw positive conclusions from the assertion or from the denial by adult subjects of such possible explanations; for, on the one hand, it is entirely probable that many actual associations are forgotten, while conversely, it is possible that plausible explanations are imagined and then assumed in good faith as the actual ones. It is at least certain, as the following table shows, that almost all color-associations and forms date back to childhood.

SUMMARY VIII.

Beginning of Pseudo-chromesthesia and of Forms.

	In Childhood.		Later.		Time?[1]	Both[2]	Total	Unv.	Total Cases
	Surely.	?[1]	Surely.	?[1]					
Pseudo-chromesthesia began	29	1	1		12	2	45	0	45
Forms began	48	7	3	3	21	3	85	0	85

In the case of pseudo-chromesthesia, the number of explanations actually offered are recorded in

SUMMARY IX.

Explanations of Pseudo-chromesthesia.

	Explanation		Total	No Explan.	Unv.	Total Cases
	Sure.	?				
Color with words (including some letter and music-associations).	3	10	13	12	2	27
Color with words only.	3	4	7	3	1	11
Color with letters only.		1	1	1		2
Color with music only.	3		3	2		5
Totals.	9	15	24	18	3	45

[1] Doubtful.

[2] Part of the color-associations (or forms) in childhood; part later.

This result is less significant than it appears to be. The greater number of explanations are, as is shown, doubtful or partial, including such as these: "When I was three years old, I had a playmate named Ethel, who had the loveliest blue eyes I have ever seen. She made such an impression upon me, that now the word Ethel seems almost a synonym for blue;" and this other: "'Harry' may be 'yellow' because associated with an imaginary Harry, with yellow curls." Color-associations with music seem most easy to explain and are in great number accounted for through a sort of emotional middle term; the connected color and sound are those which are apt to occasion similar emotions. A typical instance of this emotional connection between music and color is this one: "Color-hearing of musical sounds is due almost entirely to emotion. When I hear that which produces a pleasurable emotion, I immediately prolong this by seeing those colors which would produce the same emotion." "The tone of a violin," another says, "is very pleasurable, and blue is my color for my happiest moments." The explanation is still more detailed in the following quotation, which, however, suggests rather a deliberate connection: "As one naturally translates a lovely thought of one language into another, so the beauty of music was expressed in color . . . . I am naturally fond of red, which forms the foundation of my musical association, intensified into black and etherealized into pink; mixed with blue, for the passionate purple, and bothered with yellow to make the sullen and bitter discord of vermilion." The exact figures are these:—

SUMMARY X.

Explanation of Musical Color-associations.

Emot'l Ass'n.	Ord'y Ass'n.	Both.	No Explan.		Total Rec.	Unv.	Total Cases
			Sure.	?			
9	1	4	7	1	22	1	23

It is significant, on the other hand, since color with letters (regarded as sounds) seems obviously a simpler and more primary sensational experience than color with words, that we have but one direct explanation, and that a doubtful one, of a letter-color. This is given by a person who connects ĕ with yellow or red and ē with green, and the suggestion is that the pronunciation, spelling and meaning of the words

green, red and yellow may be the cause of the color-association. Now, most instances of pseudo-chromesthesia include both letter-color and word-color, and in half of these the word-color is at least partly explained by some ordinary association. It is not unlikely, therefore, that the letter-color is often the secondary experience and that letters take their colors from representative words. The figures are these:—

SUMMARY XI.

Connection of Letter-color with Word-color.

LETTERS ARE	CONNECTED WITH WORDS.		UNCONNECTED WITH WORDS AND		TOTAL.	UNV.	TOTAL CASES.
	Explained words.	Unexplained words.	? Explained	Unexplained			
	13	11	1	1	26	2	28

The lack of uniformity already noticed in the color-associations of different subjects with the same letters, implies, of course, the individual nature of the association and may point toward this same explanation of letter-colors through word-colors. The theory of Mr. Stevens[1] best suggests the possible connection between these colors and the childhood experience. He supposes that the color of each letter may be that of an object of whose name this letter is the initial. It is undoubtedly true that to all children the familiar horse, dog or bird is the typical one. If the sound of the word "dog" first suggests to an imaginative child his father's Irish setter, then the sound may be closely connected with the color brown; when the letters are learned, *d* stands for dog and takes on the color of the "dog par excellence." It must be remarked that this theory, ingenious as it is, still accounts with difficulty for some of the phenomena, for instance, for the vivid colors, red, green and yellow of so rare a letter as *q*; or for the red, black and yellow of *z*.

Definite explanation of forms, by identification with the shapes of familiar objects, occurs rarely in our records. Including even doubtful cases, only about twelve per cent. of our forms are explained.

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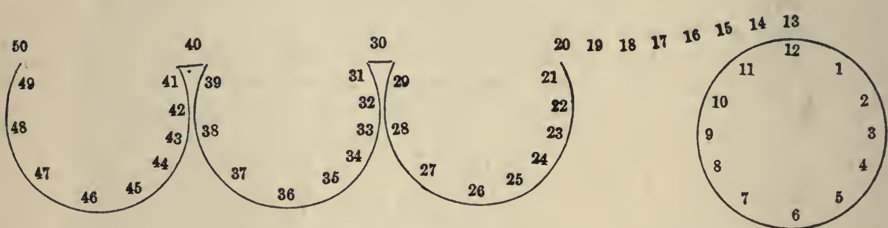
[1] *Pop. Sci. Mo.* March, 1892.

SUMMARY XII.

Explanation of Forms.

	EXPLANATION.		TOTAL.	NO EXPLANATION.	UNV.	TOTAL CASES.
	Sure.	?[1]				
Month-forms.	6	5	11	57	5	73
Number-forms.	7	4	11	49	7	67
Day-forms.	1	2	3	44	3	50
Century-forms.	1		1	10		12
	15	11	26	160	15	202

Yet most of these explanations seem more obvious and satisfactory than those offered in cases of pseudo-chromesthesia. The following, for instance, has an interesting history:



The subject says: "I cannot explain the origin of the almost straight lines between 12 and 20, but the curves came from the fact that I learned to tell time before I learned to count, and when I did learn, everything reverted to the picture of that old clock."

It is evident, however, that if the psychical theory were supported merely by the fact of these remembered associations, it could claim little value. But the natural childhood associations which it hypothesizes may certainly have existed, though they are now forgotten. An observation of the forms themselves shows that they are chiefly those of ordinary objects, always frequent and sometimes prominent in the child's environment. The classification is the following:—

[1] Doubtful.

SUMMARY XIII.

Nature of Number-forms.

Simple Straight Lines.	Broken Lines.		Curved Lines.		Total Rec.	Unrec.	Total.
	Simple.	Complex.	Simple.	Complex.			
17	15	16	7	5	60	7	67

Of Month, Day and Century-forms.[1]

	Straight Lines.		Circular.		Rectangular.		Total Rec.	Unrec.	Total
	Straight	Broken	Circles	Curved	Squares	Rectangles			
Month-form.	7	3	23	22	7	6	68	5	73
Day-form.	21	7	1	8	—	10	47	3	50
Century-form.	4	3	—	2	1	1	11	1	12
Total.	32	13	24	32	8	17	126	9	135

A classification of the points at which number-forms bend also bears on this probable explanation by showing that three-fourths of these turns are at numbers which are prominent in early arithmetical exercises and in ordinary usage.

SUMMARY XIV.

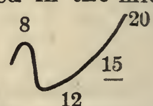
Turns of Number-forms.

At 5 and its multiples,	1			
At 10, and at 10 and multiples,	17			
At 12; 8 and 12; and 10 (and 10 with multiples),	8			
At 20; 20 and multiples of 10,	5			
At 100 and multiples,	1			
Combinations of these forms,	5			
Turns at each number,	1			
Irregular turns,	8			
Total,		46		
No turns,		13		
Total recorded,			59	
Unrecorded,			9	
Total,				68

[1] For examples of these forms, Cf. Figs. 1, 2, 4, Plate I.

Among the eight under the head "irregular turns," occur some so peculiar in form as to defy all identification with childhood objects, yet even these include many bends at the familiar 10's and 12's; and it is not unlikely that turns at such numbers as 6, 8, 15, 19 and 27 might be explained by some fact of great importance to the childish mind, by the consuming admiration for a playmate who is eight years old, or by the impressive death of some one at twenty-seven.[1]

The argument from utility,[2] which treats the "form" as a survival of a useful device, may finally be emphasized in support of the psychical theory. The visualization of numerals or of word-series may be an important aid to memory, especially in a child's first struggle with numbers. Accidental associations of this sort may then be perpetuated because of their helpfulness. This genesis of forms is explicitly recognized by one subject, who says, "the other forms have arisen simply from the ways that I have used to remember." There are many more emphatic assertions of the usefulness of forms. "I cannot realize," one man writes, "how any one can dispense with something of the kind. It seems to me that without this form, numbers would have no meaning, and a person would be entirely lost in considering them." "In the study of history," a student says, "I always associate events or men with the century which stands in such a place on the form. For instance, Queen Elizabeth reigned in the middle of the fifteenth century, which stands thus



"

"When I add numbers," writes another, "I invariably think of my form, *e. g.*, if I add 5 to 27 . . . I feel as if I passed on a step. When I multiply, I feel as if I jumped, as it were, from one place to another, *e. g.*, 6x9 is a long jump compared with 3x6." Even the musical memory may be aided. "If I hear an opera," one subject says, "I can come home and almost play it by colors; I know what chords make a certain combination of colors."

The rare occurrence of distinct alphabet-forms may be explained on this principle as due to the fact that "the child needs no mnemonic system by which to learn the letters. He already sees them before him on blocks or in picture-books. Moreover, the alphabetic sequence is not so important as the numerical sequence."[3]

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[1] Some such irregular forms are shown in Figs. 5 and 6, Plate I.
[2] Cf. G. W. T. Patrick, *Pop. Sci. Mo.* Feb. 1893.
[3] Extract from an essay by Blanche L. Clay.

The following table shows the relative number of those that are sure that they have been helped by the possession of forms :—

SUMMARY XV.

Utility of Forms.

	Utility.			No Utility.		Unv.	Total Cases.
	Sure.	?	Tot.	Sure.	?		
Usefulness of month, day and century-forms, in remembering dates and appointments.	27	4	31	39	5	3	78[1]
Usefulness of number-forms both in remembering dates, etc.,[2] and in mathematical operations.	29	5	34	21	5	7	67

Whether or not one admits, as I have been arguing, that the forms which correspond with familiar shapes and those which are of acknowledged use to the possessor, as well as those which admit of definite explanation, are likely to be due to psychical associations,—it is yet worth while to observe that all these forms may be classified under one, at least, of these classes, while most belong to more than one.

The helpfulness of pseudo-chromesthesia is less obvious, yet it is reported in about one-fourth our cases. "Colors do not look right," one subject writes, "unless a word is spelled right. For instance, I spelled permanent, the other day, with two *a's*, and it did not look pale enough." Another is helped in writing rhymes, and a third in committing music to memory.

The pleasurableness of the experience also is very general, and may be a reason for its perpetuation. In several instances, already quoted, the favorite color has been definitely recognized as the basis of all the color-associations. The positive pleasure of this color-experience is naturally greater, as appears from the last part of the following table, than that derived from the usually simple and pre-eminently useful "forms."

[1]The number refers to subjects, not to different forms.

[2] In the cases in which number-forms are used as year-forms or as day-of-the-month-forms.

Summary XVI.

Psychical Value of Pseudo-chromesthesia.

	Yes.			No.		Neither	Both Diff't Times	Total	Unv.	Total
	Sure	?	Total	Sure	?					
1. The subject's memory is helped.	10	3	13	30		1		44	1	45
2. The subject is fond of the associated colors.	13	8	21	12	2	8		43	2	45
3. The subject finds pleasure in the experience.										
(a). Of pseudo-chromesthesia.	14	11	25		1	13	4	43	2	45
(b). Of forms.	24	9	33	1	5	46		85		85

The diminution of both phenomena is slighter in our subjects than is usually supposed, perhaps because of their comparative youth; it suggests the effect of lack of attention when the experience has become monotonous and has outlived its usefulness.

Summary XVII.

Relative Increase and Decrease

	Increase.		Decrease.		Both.	Neither	?	Total	Unv.	Total.
	Sure.	?	Sure.	?						
Of pseudo-chromesthesia	4	2	9	3		18	7	43	2	45
Of forms.	14	5	6	6	1	39	14	85		85

The hereditary tendency of colored-hearing and of forms indicates, of course, the importance of the accompanying cerebral changes. Including doubtful ones, the number of negative cases is probably far too high,[1] since in many cases one person has answered for an entire family, while, in almost all, there has been no opportunity for careful investigation.

[1] The cases under "No?" are merely those in which the subject has no knowledge that members of his family have forms, etc.

SUMMARY XVIII.

Hereditary Tendency.

Do members of a family have

Answers.	*a.* Pseudo-chromesthesia? Yes. Sure	Yes. ?	No. Sure	No. ?	Total.	Unv.	Total Cases	*b.* Forms? Yes. Sure	Yes. ?	No. Sure	No. ?	Total.	Unv.	Total Cases
1. Of subjects with pseudo-chro. only.	2[1]		9	10	21	1	22	0		8	12	20	1	21
2. Of subjects with forms only.	5		14	42	61	1	62	24	24	6	7	61		61
3. Of subjects with both.	5	1	4	13	23		23	7		4	13	24		24

The remaining tables embody the answers to questions of less importance, yet of a certain interest. It is not surprising to find that nearly all our subjects are good visualizers, but it is more remarkable that nearly all of them draw or paint.[2]

SUMMARY XIX.

Colors with Odors, Tastes and Touches. Visual Imagination.[3]

	Colors with Odors, Tastes and Touches. Yes. Sure	Yes. ?	No.	Total.	Unv.	Total Cases	Visual Imagination. Yes. Sure	Yes. ?	No. Sure	No. ?	Total.	Unv.	Total Cases
Subjects with pseudo-chro. only.		1	17	18	3	21	11	7		1	19	2	21
Subjects with forms only.	1	2	58	61		61	52	5	3	1	61		61
Subjects with both.	1	3	20	24		24	23			1	24		24

[1] Color-association with odors.

[2] This question was asked by MM. Beaunis and Binet of their subjects. Cf. *Revue Philosophique*, April, 1892.

[3] See note 3 on page 454.

SUMMARY XIX.—*Continued.*

Drawing or Painting.[3] Dramatization.[4]

	Drawing or Painting: Yes.						Dramatization: Yes.		No.				
	Sure.	A little.	No.	Total.	Unv.	Total Cases	Sure	?	Sure	?	Total.	Unv.	Total Cases
Subjects with pseudo-chro. only.	5	4	10	19	2	21							
Subjects with forms only.	16	11	34	61		61	7	4	72	1	84	1	85
Subjects with both.	7	11	6	24		24							

Under the head "Dramatization" are included some simple cases in which, for instance, certain numbers or days are essentially disagreeable or "sharp and keen." There are, besides, more elaborate personifications, like the following:—

"T's are generally crabbed, ungenerous creatures. U is a soulless sort of thing. 4 is honest, but mathematically angular and ungraceful. 3 I cannot trust, though it is fairly good-looking in personal appearance. 1 is dark in complexion. 9 is dark, a gentleman, tall and graceful, but politic under his suavity."

"For numbers, I entertain either a like or a dislike; for instance, 11, 13 and 17 are especially disliked, I suppose because they are prime. My feeling for 11 is almost one of pity."

"The letters are very individual, for instance, K seems like a young woman, a friend of L, which seems like a daughter to M. N seems to be a sort of maiden aunt, sister to M. O is a young man connected with M as a nephew. He connects M and N with P, an older friend of his. Q is odd and stands by himself as rather an eccentric middle-aged man. R is like a maiden lady, an advisory friend of S, a young, handsome girl. T is the devoted admirer of S."

To one person, written capital letters have different expressions according as they are made in different ways, for

[3] In the investigation of forms, both these questions were divided, with the following result: One-fourth of those with good visual imagination of form do not strongly visualize color; and about one-ninth of those who draw do not also paint.

[4] I am inclined to think that this summary over-states the negative, for those who answered the question in writing may have misunderstood it. The question was not asked of subjects of pseudo-chromesthesia.

example, one style of written *I* is "honest and well-intentioned, but dictatorial and overbearing," while, written in another fashion, *I* is "a crabbed old miser." The subject adds: "This feeling is so strong that at times, when I have wished to produce a certain impression by a certain sentence, I have noticed that the capital letter at the beginning looked so at variance with the tone of the sentence that I erased it and made it in a different way, in order to keep the harmony."

The results of this paper, so far as they are not merely a summary of statistical observation, may be briefly summarized: We have no direct proof for the psychical theory of forms or of pseudo-chromesthesia. An examination of the different forms shows us, however, that most of them may be plausibly explained by the hypothesis of forgotten childhood-associations; and this probability is increased by the fact that such associations would be useful in learning the number series and in remembering dates.

Musical color-hearing and some name-associations are explained in the same way. Color with the letters may also be accounted for by arbitrary and forgotten childhood-associations; but it is possible that the explanation in this case is primarily a cerebral one. In general, however, such color-associations are either useful or pleasant, so that, even if their occasion be cerebral, their continuance, both in the individual and in the family, is largely due to attention and to cultivation.[1]

APPENDIX.

The descriptions, which follow, of special cases are extracts (except the final one) from essays by different students. The last two are records of self-observation; the others embody the results of personal investigation.

Pseudo-Chromesthesia.

Case A.

Miss A. is a girl about nineteen years old, who says that she has had this experience ever since she can remember, but that it has never occurred to her as anything unusual. She sees the color only when she hears the letter or word, that is, when someone else speaks it; when reading, unless she stops to say the word to herself, she has no impression of color.

The phenomenon manifests itself with her, especially with the letters, both vowels and consonants, and with words only in so far as the initial letter throws the color over the rest. For example, as *a* is blue, Alice is blue, and because *s* is yellow, Sunday is yellow. The same rule holds good of figures; 2348 would be red, because *2*

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[1] I am indebted for many suggestions in the collection of statistics and in the discussion of theories to my friend and former student, Miss Helena M. Corey.

is red. There are, however, some exceptions. When there are two consonants at the beginning of a word, their colors frequently blend, producing a color which is a combination of the two, for instance, in the word Thursday, the color is a red-brown, a combination from *t* which is red, and *h* which is brown. Moreover, there is a strange exception in the name Monday, which appears blue, while *m* is a decided red. Miss A. says that she cannot account for this, unless the association of blue with Monday in "blue Monday" replaced the original color red, and became more firmly fixed in her mind than the color produced by *m*, the initial letter.

There seems to be no especial arrangement of the letters according to color, although the same color is often repeated in the course of the alphabet. For example, *d*, *j*, *n* are all brown, whereas *f*, *k*, *w* are steel-gray. Each letter has a distinct color of its own, apparently without reference to form or sound. But it sometimes happens that letters with somewhat the same form have the same color, for instance, both o and *c* are white, and the figure zero is white also, while the figure *1* and the letter *i* are both black, but *3* and *6* are black also, and they differ so totally in form that the rule will not hold good in their case.

To illustrate further that the sound of the letter has no influence, I asked Miss A. about the colors of "ought" and "aught," and she said that "ought" is white, while "aught" is blue, following the rule of the initial letter.

The colors corresponding with the letters and numerals are as follows:—

a, *b*, and *8*, blue.
e, *u*, *g* and *x*, tan or dark écru.
i, *h*, *p*, *1*, *3*, *6*, black.
o, c, *0*, *10*, white.
d, *j*, *n*, *9*, brown.
f, *k*, *w*, *11*, steel-gray.
l, *r*, *z*, steel-blue.
m, *t*, *2*, *5*, red.
q, *s*, *v*, *y*, *4*, 7, yellow.

Most of the colors, it will be observed, are of the softer, more sombre hues. Some of them, however, are more distinct than others, for example, the red, blue and black, of which the red is by far the most vivid, apart from its being the brightest color.

But the more subdued colors, especially those in the écru shades, are almost indescribable. They are so vague and indefinite that it is hard to give them any name.

Miss A. does not usually see the letters or words themselves colored; she has merely a vaguely outlined image in the characteristic color, when the word is spoken, for instance, with the word "other," she sees a sort of "streak" of white. But the very vivid colors affect the letters themselves, for example, *m* and *t* always seem red to her.

The subject has no explanation to offer. She thinks, however, that her colored-hearing cannot be the result of early association, because the colors have been produced only gradually, and have increased with time instead of fading, as might more naturally be the case if this were a mere childhood-association.

MARY L. SMITH.

CASE B.

Miss B. is also about nineteen years old. She has had colored-hearing ever since she was a child, long before she could either

read or write. With her, however, colors were first associated with names and only later, since she has thought about the matter, has she associated colors with the letters of the alphabet. B.'s mother first noticed the peculiarity, when she was asked to suggest names for the marbles with which the child was playing. The mother proposed names, but B. rejected most of them as unsuitable, because, she said, they were not of the same color as the marbles. This incident impressed the mother as very curious, for she does not know that any others of the family have colored-hearing. She had never, however, spoken of it particularly until B., who did not remember the experience, began talking about colored-hearing, a few weeks ago.

Miss B.'s colors, for the letters, are these:—

a, blue.
e, yellow.
i, dark red.
o, light gray.
u, scarlet.

b, black.
c, yellow.
d, dark red.
f, yellowish-brown.
g, brown.
h, cream.
j, brown.
k, dark blue.
l, red.
m, brown.
n, deeper brown.
p, dark blue.
q, dark red.
r, brown.
s, dark blue, almost black.
t, still darker blue.
v, dark navy-blue.
w, red.
x, green.
y, cream-color.
z, dark, almost black.

Miss B. sees colors with all letters and with almost all words, but the association is most marked with proper names. The following are her "month-colors":—

January, red.
February, straw-color.
March, blue.
April, purple.
May, gray.
June, red.
July, deeper red.
August, sky-blue.
September, brownish-yellow.
October, light yellow.
November, bluish-gray.
December, reddish-brown.

The word may be spoken or written; but the color does not always appear with an imagined word. No other sounds of the human voice or of musical instruments ever suggest color. There seems to be no rule, such as that of the initial letter, for the association of colors with words, but each separate word and letter has its distinct color, and if the colors seem to be duplicated there is a decided difference in the shade. For instance, Sarah and Stella, which have the same initial letter, are totally different in color, the former a bright blue, and the latter corn-color. Harry, which has almost the same sound and in great degree the same letters as Carrie, is dark red, while Carrie is a very dark blue, almost black. If we compare these with the colors of the letters, we still get no explanation of the coloring. MARY L. SMITH.

CASE C.

Miss C.'s colors for vowels are as follows:

a, dull gray.
i, nearly black, very dark green.
o, nearly colorless.
e, dull red.
u, dark bottle-green.

Her colors for consonants are these:—

b, brown.
c, vivid lemon-yellow, the brightest letter of all.
d, dark brown.
f, ruddy brown.
g, purple.
h, greenish-yellow.
j and *k*, no color.
l, dull gray.
m, brown, especially distinct.
n, nut-brown.
p, lead-color (tint of purple).
q, dark green.
r, deep brown.
s, white with brown spots; (in combination) pink.
t, bright green, a very clear color.
v, fawn-color.
w, heliotrope.
x, black.
y, mixture of lavender and blue, Dutch-blue.
z, yellowish-drab.

It is noticeable that many of Miss C.'s colors are of a dark shade; she says that the colors were much more distinct in childhood and it is possible that they have lost something of their original brightness. Upon being asked whether the impression of color comes when the word is merely heard, or when read and imagined, Miss C. replied that it comes when imagined and when heard, never when the printed page in black and white is before her, unless she stops to imagine the word or letter. The black and white seem to dispel the colors. The colored spoken letters do not seem quite so distinct as those which she imagines. No other sounds than those of letters and of words produce any color, and the quality of the voice does not make the slightest difference in the colors, which are the same in connection with all voices. Her own moods have no effect on the colors.

The separate letters of a word are all themselves colored and on a rather dark background. In most cases the color comes from the first letter, but very often the word is shaded; this effect does not usually come from combination, but each letter is seen as a separate one, in its own color. These statements have been verified. The word "Carrie" appears to Miss C. a bright yellow, influenced no doubt by the *c*, which is lemon. "Harry" is greenish-yellow, the exact shade of *h*. Here the similar sound of the words (irrespective of the initial letters) seems to have no influence. "Helen" is dark green, influenced by *h* and perhaps somewhat by *l*, a dull gray; this seems like a combination of the two colors. "Stella" is one of the few shaded words, a very delicate pink, shading into a dull green. I am sure that this comes from the combination, for *s* (white with brown spots, when isolated) is invariably a very dull or delicate pink when with other letters; *e* is a bright green, and in combination with *l* and *a*, dull gray, would very likely give dull green.

The subject occasionally finds that in trying to think of a word, the flash of color comes just before the word, though, until questioned, she had never thought of the significance of this experience as an aid to the memory. These flashes, however, occur very seldom. The word-color has existed ever since she can remember; in childhood, it was often a source of amusement and she never doubted that other people associated colors with words and letters. She is the only one in her immediate family who has colored-hearing; until her attention was called this spring to the peculiarity, she had never mentioned it at home, and her family were much surprised at her experience.

Miss C. can offer no theory of colored-hearing, but the fact of having learned one's letters from blocks is not a satisfactory

explanation, since it would account for only a few colors, which should then be often repeated with the different letters.

AGNES M. SHAW.

CASE D.

The subject is a girl about nineteen years old, who has always been in the habit of seeing colors in connection with all letters of the alphabet, including Greek letters, with words made up of these letters and with figures. The complete list of the associations follows:—

a, light brown.
e, reddish-yellow.
i, black.
o, white.
u, cloudy white.
b, dark brown.
c, white.
d, dark blue.
f, brown (*F* gray).
g, dark blue.
h, brown.
j, black.
k, light blue.
l, blue-black.
m, brown.
n, gray.
p, dark blue.
q, blue-black.
r, grayish-white.
s, reddish-yellow.
t, blue.
v, dirty white.
w, brown.
x, red.
y, grayish-white.
z, red.

The words and consonants seem to be about alike in clearness, but the capital letters, besides being larger, are more distinct and appear brighter and more conspicuous than the others; sometimes they even assume different colors or shades, as in the case of small *f*, which is brown, while capital *F* is gray, or, as in the case of small *q* and capital *Q*, which are different shades of blue. There is also a marked difference in shade between written and printed letters, so that ℓ, *l*, $\mathscr{L}$ and *L* have different shades. Miss D. thinks that the distinction is due to their difference in shape.

There seems, however, to be no difference in shade, according as the letters are spoken or written. If a word is spoken quickly, it immediately assumes the color of the initial letter, but if the word is imagined, or slowly repeated or read, then not only the initial letter, but all the letters assume colors, so that a printed page seems to be illuminated; the letters are not on a colored background, but each stands out for itself in its own individual color. (Therefore, if, in an illuminated text, letters thought of by the subject as, perhaps, brown and white, are made, for instance, red and black, they do not appear natural, and must either be re-colored or be printed in ordinary form to suit her taste.)

The subject has no explanation or theory of her colored hearing. The experience is not hereditary and is not connected in any way with childish associations of which she is conscious. She does not think that the colors can be closely connected with the sounds of the letters, for two letters sounding precisely alike are represented by very different colors, and two words pronounced alike, but spelled differently (as *aught* and *ought* or *air* and *heir*), appear very dissimilar when seen in colors. She thinks the color more likely to be associated with the form of the letters; yet one can detect no similarity of form in *a*, *b* and *m*, which are shades of brown; or in *d*, *k*, and *p*, which assume the various shades of blue.[1]

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[1] There is, however, such similarity between *b*, *f*, *h* or between *m* and *w* (brown); and between *g*, *p*, *q* (blue).

Miss D. has a use for her colored hearing. She says that the practice of associating colors with letters aids her greatly in correct spelling and in committing words to memory.

MARY R. EASTMAN.

CASE E.

The subject is eleven years old and has had colored hearing ever since she can remember. All the letters of the alphabet, the names of days of the week and of months of the year, numbers and many Christian names, but no common names, are colored. The letters, either separately or occurring in words, are not themselves colored, and the image of the color is vague in form. Both sound and sight, but the sound more clearly, suggest color.

The list of letters with their colors is this:—

a, black.
e, grayish-white.
i, gray.
o, white (more of gray).
u, gray.

b, blue.
c, black.
d, blue (a little different from *b*).
f, dark red or black,
g, grayish-black.
h, blue.
j, bluish-black (more black).
k, red.
l, fast black.
m, a blue-and-red.
n, a blue-and-red.
p, red.
q, gray.
r, reddish-brown.
s, bluish-black.
t, brown.
v, gray,
w, reddish.
x, black.
y, brown.
z, black.

The colors of names of days are:—

Sunday, black.
Monday, reddish-brown.
Tuesday, blue.
Wednesday, whitish-gray.
Thursday, whitish-gray.
Friday, blue.
Saturday, whitish-gray.

The month-colors are:—

January, grayish-white.
February, reddish-brown.
March, black.
April, dark brown.
May, pink.
June, pinkish.
July, black.
August, grayish-white.
September, reddish-brown.
October, dark blue.
November, dull brown.
December, blackish.

The colors with numbers are:—

0, grayish.
1, gray.
2, brown.
3, very dark blue.
4, light.
5, reddish-brown.
6, black.
7, brownish.
8, blue.
9, black.
10, reddish-brown.
11, gray-white.
12, gray-white.
13, dark-blue.
14, yellowish.
15, reddish-brown.
16, black.
Etc.
21, brown and gray (with colors distinct).
Etc.
30, dark blue (but lighter than 3).
Etc.

It will be observed that 0 by itself is gray, but, occurring with the other numbers, unites with the color of the accompanying number, making it lighter.

My subject paints well in water-colors, and is a fairly good visualizer. I have tried her several times, unexpectedly always, and she has invariably given the same color for the same sound, though the colors are often difficult to describe. They are frequently modified shades or a mixture of several colors. Indeed, in almost all cases of colored-hearing, it seems to be impossible for the subject to indicate the exact shade which a given sound brings to her mind. The color is, for instance, reddish-brown or grayish white more often than it is clear red, brown, gray or white. If the colors, as she describes them, are put upon paper, they never quite satisfy her.

My subject can not account for the phenomena of colored-hearing in her own case. She says that she "feels queer" if the colors do not come as soon as she hears the words corresponding with them. I can not myself determine any rule which governs her color-associations. The initial letter does not often color the whole word: Wednesday is not the color of *W*, nor Oscar of *O*. Neither do the vowels give coloring to the word, and the words do not seem to be the result of the mixing of the various colors of its component letters. But rhyming words, for instance, Harry and Carrie, frequently, though not not always, suggest the same color, indicating again that the color is directly connected with the sound.

BLANCHE L. CLAY.

NUMBER-FORMS.

CASE F.

(The illustration which accompanies this description[1] is from a stereoscopic photograph of a twelve-foot wire, bent by the subject into the characteristic form. The various rests and cords which complicate the picture were necessary to hold the wire in place. The form is represented by the heavy line. The accompanying description is written, as appears, by the subject herself.)

I have had my number-form since early childhood. I can not remember when it began, neither can I remember when and how I learned to count. I have a dim recollection of being set to learn the addition table and of making use of the form, which was even then in existence. It occupies a subordinate place in consciousness, and, though always present when the subject of thought has to do with figures, it may not be distinctly imaged. For instance, I always make use of my number-form in solving a problem, because it helps me, but my mind is closely occupied with the problem and is only dimly conscious of the form.

The numbers, which run in a general north and south direction, are not all in the same plane, and my position with respect to them frequently changes. I usually seem to stand just outside the line of numbers, and *near*, *but never on* the number most prominent in my mind at the time. I feel much more at home among the numbers about 20 than in any other place on the form. Generally speaking, the plane of the number rises from 1 upwards, but there are many small ups and downs in the line. From 1 to 5 the course is downward; at 6 comes a sharp and upward turn; 8 is lower than 6 and 7 and is in a corner. From 8 to 12 the course is upward, with a bend at 10. At 12 comes another sharp turn; from 12 to 19 the line descends, making a sudden descent at 16, and a steep ascent at 19.

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[1] Fig. 1, Plate II.

The line bends slightly at every number between 15 and 19; 18 is sunk down in a corner, a little like 8. 20 is higher than 19, and at 20 comes another bend. 21 is on a level with 20. The numbers 21-29, 31-39, 41-49, etc., follow the same order as those from 1 to 9. 30 is higher than 20, and may be seen from the latter number, since the intervening numbers are in a lower plane. 40 is lower than 30, and is at its right. 50 is higher than 40, and 60 is higher than 50, but lower than 30, which is the highest number in the line. Both 50 and 60 are to the right of 30. At 60 there is a turn to the left. 70 is lower than 60, and 80 is lower than 70. At 80 there is a southward bend. 90 is as high as 60, and is situated in a bend in the line. 100, which occurs in another bend, stands east of 90, and is almost as high as 30.

Outside the form on all sides is darkness; the line itself grows dusky at 70, and beyond 100 there is total darkness, except when that part of the line is directly fixated. The numbers between 100 and 200 run in the same order as those between 1 and 100. This is true also of the numbers between 200 and 300, 300 and 400, etc. The numbers 100, 200, etc., up to 1000, that is, the even hundreds, follow the same order as the numbers between 1 and 10. The same is true of the even thousands. 1,200 and 12,000, 1,500 and 15,000, are like 12 and 15 respectively. The numbers between 1,000 and 2,000 follow the same order as the numbers between 1 and 1,000, etc. Beyond the thousands the form grows dim and disappears. From 1 to 10, and from 20 upward, the numbers seem more or less in shadow, when not distinctly imagined, but between 10 and 20 there always seems to be a bright light. These latter numbers occupy a larger place than any other system of tens in the line, although the numbers between 1 and 10 are rather spread out. The 2's in the 20's and the 3's in the 30's, etc., help to fill up space and make the numbers seem more crowded together. In still higher numbers the hundreds and thousands take the attention to some degree from the figures in the units' and tens' places; thus in 178, I think of the 100 as much as of the 78. The 100 does not occupy the mind enough to get placed in any form, but it prevents my giving 78 its place in the line as quickly and as vividly as I should if it stood by itself, without the 100.

From one number in the form I can often see others at a distance. From 1 I can dimly see 10, and from 10 I can plainly see 20, 16, and a few others in the vicinity of 15; 13 is partially hidden from 10 by 12. From 12, which stands almost as high as 30, I can indistinctly see the latter number and 40, though many of the intervening numbers are hidden from view. From 30 I can look across to 90 and 100, and even beyond.

Each number in the 'teens seems to occupy a comparatively large place in the form, which curves in passing from one number to the next. This is partially true of the numbers between 1 and 10. The order in the 'teens is unlike that in any other part of the form, except when the 'teens recur, as in 113, 213, etc. All the numbers seem to be printed, and lying down on a dim background.

I associate character and sometimes sex, generally female, with numbers. 1, 2, 4, 7 and 8 are reliable, quiet, well-disposed, but not brilliant numbers. 3 is a sharp, shrewd, noisy and disagreeable number, always making as much trouble as possible. 5 is sprightly and merry, happy, and a number to be petted. 9 is dignified, though a little like 3. 10 is well-disposed and dignified. 11 is rather disagreeable, but not troublesome. 12 is a dignified, protecting number, capable of ruling all the lower numbers, even 3, and always treated with respect by the larger numbers. For 13 I

always have a great antipathy. It has all the disagreeable qualities of 3 added to a pertness and aggressiveness which make it repugnant to all the other numbers, with which it seems never to associate. I never wanted to be 13 years old. 14 is like 4. 15, although like 5, always seems strange, irregular, and out of place in the system. I frequently forget it in counting. I feel as if I had to go out of my way to bring it into the form at all. It seems entirely unmanageable. I always feel a great respect for 16. All these numbers are dignified and well-disposed, not brilliant. 18, however, is more important and occupies a larger space than 20.

The numbers in historical dates I think of in a way different from that in which I think of those in the ordinary number-form. The numbers which represent the century in which an event occurs are quite prominent in early dates, *e. g.*, in the date 1020, I think scarcely at all of the 20, but the 10 seems to occupy the place occupied by 10 in the ordinary number-form; I do not think of it as 1000, or even as 10,00, but simply as 10. If I think of the date sometime, or in connection with other dates in the same century, my attention becomes fixed on the last two numbers, which take their ordinary places. The nearer the date approaches the present, the more I think of the last two numbers. The numbers in centuries previous to ours seem to occupy a smaller space than the corresponding numbers in our own century. 1867 seems comparatively near. 1767 does not occupy a space analogous to 1867, but seems to occupy one analogous to 1857, etc. I never think of historical dates in connection with 1000. It is never 1,867, but 18,67.

BLANCHE L. CLAY.

CASE G.

The bent[1] wire is as good a material representation as I can give of the fact that successive numbers from 1 upward to 100 have a space relation to each other, such that if I consider the succession as a *whole*, I am conscious of mentally glancing along an even but more or less abruptly turning path, tending upwards at about 45°, but growing steeper among the upper numbers. In this glance over the whole series I locate myself more or less definitely in two positions: at a point about half way between 0 and the point on the base directly beneath the 30, for the early numbers, but for those above 25, at a point in the vicinity of 20 (a little in front of the place for 20, as seen in the cut); nevertheless, I reserve the right, as it were, to take other positions, for I find that I often consider limited portions, *e. g.*, 70–80, from a nearer point of view. From 50 upwards the form is not as definite as below, and between 50 and 75 it is not as definite as from 75 to 100. In counting beyond 100 a mental tally is kept of the hundreds, and the fractions are repeated along the form; the "mental tally of the hundreds" is quite closely associated with the printed figures 200, 300, and so on for higher numbers.

Multiples of 10 are naturally the most prominent positions along the form, and in a rough way the straight lengths between the bends include ten units, though at 30 there is hardly a perceptible turn, and the exact turning points are not at 10, 20, etc., but at 12, between 18 and 20, and between 40 and 42, thus suggesting an association with multiples of 6. Above 50, though not as definite, the turning points are at 60, 70, etc. The length also of the upper decades, as if foreshortened and distant, are less than of the lower.

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[1] Fig. 2, Plate II., from a stereoscopic photograph.

[Owing to the position of the form relative to the camera, the bend at 19 does not appear as distinct as it is. The line from 20 to 40 is in a vertical plane nearly parallel to the vertical plane through the line 0 to 12.]

I can not explain the form from any associations of drawings, pictures, things or incidents. It may, however, have resulted unconsciously from many such early associations.

While the form gives position to numbers, it does not have much to do with processes of treating numbers, except, perhaps, for adding, in which operation I follow the increasing sum along the form. 4×5, however, does not mean that I shall take four lengths of five units each and measuring them off on the form, reach 20. 4×5, 3×10, etc., are mental relations whose equivalents, 20, 30, etc., are located on the form. Moreover, in written work with figures very little reference is made to the form.

Other relations have forms in my mind: as dates, not at all connected with the number form, months of the year, days of the week; and as a rule my thoughts have a background of the mental pictures of places, things and people with which they are concerned. Dates, *i. e.*, the years, seem to slope downward away from me if they are past, and upward if they are future. The months of the year form a closed cycle, in shape a quadrilateral, its corners between December and January, the middle or last of March, the end of June, and the middle of September. Undoubtedly this is connected with school terms and recesses as well as with the seasons. Days of the week succeed each other in a straight open series.

My use of such forms in thought seems not to be related with any special trait of character, for I do not draw or paint, am not extremely methodical, and do not have more than an ordinary memory for the forms of things; yet my taste is toward applied rather than pure mathematics, and I find that I tend to give a geometric interpretation to a mathematical expression.

This habit or trait of using space-forms for thought is sometimes helpful in getting conceptions and in remembering, often it is a hindrance.

ARTHUR E. KENDRICK.

PLATE I.

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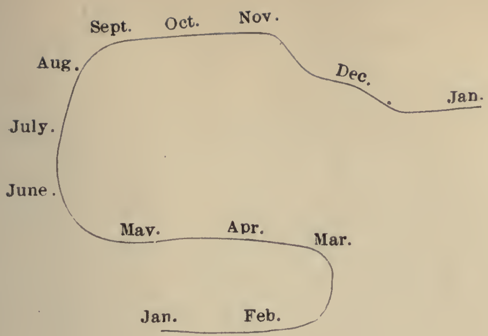
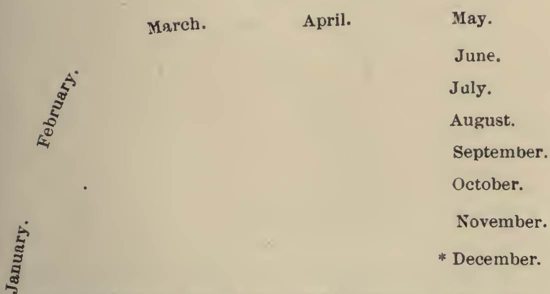
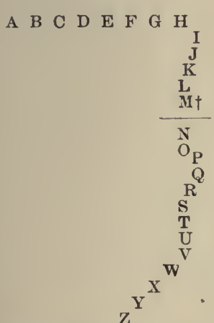


FIG. 1.



*A spring is imagined, which draws December back to the January position, for a fresh start.

FIG. 2.



† The line between M and N represents a fence between them.

FIG. 3.

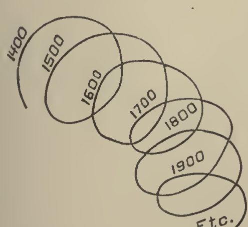


FIG. 4.

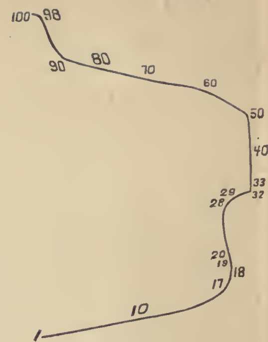


FIG. 5.

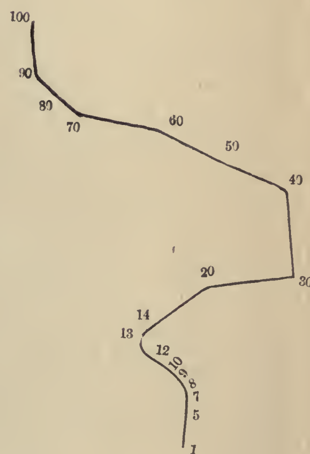


FIG. 6.

29 is sometimes associated with other combinations of circles.

FIG. 7.

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EJECTIVE PHILOSOPHY.

By Thos. P. Bailey, Jr., Fellow at Clark University.

Empiricism and intuitionalism have always been foes: the one abhors "subjectivity" without a history, the other despises uncritical "mechanism." Yet both these philosophic attitudes (for schools they are not) claim to rely on experience; and their claims are just. Both admit that there are sensations and relations between sensations. The mechanically minded see in sensations *the real* mind-stuff, and defiantly ask, what would become of your relations, your intuitions, if their terms were taken away? You acknowledge that you *feel intuitions*, but are not those feelings the practical results (for life and conduct) of the workings of the psychical mechanism? Even admitting that feeling is primary, how could you have *cognitive* intuitions if you did not have language, which is a sensational symbolism at bottom? It is anthropomorphism that makes you feel that a cause *produces* an effect. To be a logical believer in causation, you must see powers working in all natural successions. But the intuitionalist replies: Principles are the important things in life and mind, and sensations are but the handles whereby we may catch hold of intuitions. The genesis and the dressing of an idea determine nothing as to its validity. The practical results are ends, and the psychological mechanism is a means. Language necessarily has a sensational dress, for it appeals to the ear and the eye. But how about gesture and facial expression and the inherent grammatical categories which are the substance of which language is the shadow! You may slur the "ifs" and "buts" and "therefores," but you cannot *think* without them. You cannot explain away cause, because you cannot transcend your experience. You cannot annihilate reason without annihilating your cock-sure empiricism. Why has not the foolish belief in causation been annihilated by "natural selection?" You had better not talk about anthropomorphism while the sword of the idealist is bared. And so they have cried "come again!" at each other through the rolling centuries. I give merely samples of the fence-play. But Berkeley, the idealist, was a man of faith, and Herbart, the empiricist, fought utilitarianism so keenly as to stab at its masked face in the categorical Kant! How humanly precious is inconsistency in philosophy! These naïve contradictions between theory and practice, logic and instinct, have for some time been foreshadowing the larger view of a biological and anthropological philosophy. Many of the old issues are dead or dying. The empiricists are learning some lessons: However "derived," relations are now instinctive and are the most valuable of mental things; language had cognitive implications from the start, and its growth has been in a cognitive direction, however much the sensational "terms" may vary; let us hold to a chastened anthropomorphism lest the idealist and the materialist destroy us—we can think of matter only in terms of mind, and of mind only in terms of matter (Spencer); perhaps we had better ease up a little on the "prin-

ciple of relativity"—the biologically-minded have given us the better principle of fatigue, the discovery of hot and cold spots, the phenomena of monoideistic trance, etc.; the probability that there are pleasure and pain-fibers render the "theory of relativity" too metaphysical, and empiricists must beware of metaphysics, at least *old* metaphysics. The intuitionalists have also learned some lessons: Relations are not degraded by having pedigrees; empiricists are often neither fools nor knaves; physiology and biology are magnificent allies; perhaps it is better not to dogmatize too much about the origin of languages, especially as we know so little about non-Aryan tongues, the North American Indian language *Stocks*, for instance; the empiricist is unable to do any harm in his efforts to disprove causation; let us allow him to jump out of his own skin if he wants to; on the other hand, let us beware of entangling alliances with the idealist, who insists that his skin is not spacial *in essentia*, for the true intuitionalist cannot afford to part with common sense; perhaps foolish beliefs may be inherited because they are useful—perhaps we need a *critique of pure instinct*. We even begin to find a few intuitionalists that study Herbert Spencer *sympathetically*, and an occasional empiricist that has patience with the Scottish school of philosophy. But the old issue is still before us—sight *versus* faith, sensation *versus* intuition. The trend of the best equipped and most earnest minds of today is, I think, toward instinct as the starting point, and the criterion of philosophical activity. Then have the intuitionalists won? The criterion, but not the method. The empiricists have lacked faith in instinct, but they have had faith in sight. Now the method of science is to express relations in terms of quantitative sensations. Impossible as it is to weigh and measure faith, hope and charity, it is possible to objectivize, to sensationalize, or at least to symbolize in terms of the biological sciences our psychological and philosophical knowledge. Shall not philosophy express her faith in terms of sight?

As is the psychology, so is the philosophy. Philosophy is the science of the sciences; it is the unifying, or, better still, integrating science. Psychology (in the widest sense) is the mental science corresponding to the physical sciences. Whatever else the terms of philosophy may denote, they certainly *must* represent facts of normal experience. Logical puzzles and word-mongering are not philosophy. The time for seeking *noumena* has passed, for phenomena *are* noumena, but there are phenomena *and* phenomena. Thinkers discriminate the relative and the absolute in experience: and psychology is the science of experience. But what sort of psychology? That which accepts *all* the facts of experience, does not try to explain away any class of facts, does not accuse the human race of ineradicable illusion, has no metaphysical (materialistic or traditional) theory to sustain; but which uses mechanical hypotheses wherever it can, does not worry itself about whether brain explains mind, or whether mind explains brain, interprets the results of trained and of untrained introspection, by means of objective symbols and analyses, expresses the laws of mental operations in terms of mechanism, and the validity of mental relations in terms of spontaneity. Such a psychology finds itself at home in brain-psychology and in abstract ethics, without confounding the sciences or dividing nature. Spiritualists and materialists, idealists and realists, pantheists and theists must all appeal to it, or be condemned to the nebulosity of an unscientific metaphysic. Philosophy must have a valid psychological starting point, a psychological standard and a psychological criterion. Shall we start with "sensation," or "intuition," or "feeling"(!), or "volition?" In my opinion, the

starting point is muscular strain; the standard is impulse; the criterion is catholic *human* instinct, but I must try to justify this opinion.

It seems almost like sacrilege to question the tripartite or trilogical division of mind into "intellect, feeling and will." Without attacking the time-honored division, let us adopt another for the sake of convenience. Mental facts are of two kinds: the ejective and the effective. The ejective includes sensation and relation (intuition); effective includes impulse and emotion. Sensation is of two kinds: affective (pleasure and pain), and objective (the "six senses"). Relation is of three kinds: automatic, as time, space, number, etc. (the *concepts of* time, space, etc., are not here referred to); reflex = biological, psychical, social = *animal* (including *homo sapiens*) instincts relation; intuitive = æsthetical, logical, ethical = human instinct relation. Turning now to the effective, impulse is muscular feeling with a trend toward outwardness (away from diffused subjectivity), activity in a definite direction, integration for an end. Emotion is muscular feeling with a trend toward inwardness (away from definite objectivity), reaction in a (mentally) indefinite direction, segregation for recuperation. Now, how is muscular feeling the starting point of philosophy?

Movement is the end of mind as motion is the end of matter. Matter is a "permanent possibility," not of "sensation," but of motion. Mind is a "permanent possibility," not of "having sensation," but of movement (muscular feeling). In the reflex arc the sense-organ and the nerve-centre exist for the muscular contraction. In character, knowing and "feeling" exist for doing and the capacity for doing. Muscular feeling underlies attention, resolution, determination, spontaneity; benevolence, justice, equity; infinity, eternity, absoluteness; force, power, might; personality, institutions, history. Our minds are "active" or "passive;" we "form" opinions and "diffuse" knowledge; blind deaf-mutes get along nicely with but a single *representative* sense. In the lower animals, sensations are but guides to muscularity. In us, sensations and cognitions and pleasures and pains mean nothing to us if we are debarred from bodily and mental activity and rest. Matter is symbolized force; force is objective energy (muscularity), however much you de-anthropomorphize yourself, until you call it P and put it in a formula. We cannot get rid of foot pounds and horse powers either in physics or psychology. Physics has adopted units of force, has psychology adopted units of energy? The reflex arc! Yes, and the muscular end of it. There is but one valid psychophysic fact, the contraction of a muscle *with* the feeling of muscular strain. In this phenomenon, energy is force, force is energy. The power (not noumenon) "behind phenomena" is *at least* energy-force. What are beauty and truth and right apart from *attraction!* Says Prof. Lloyd Morgan very truly and very nobly, "Knowledge and art are justified by their influence on conduct; truth and beauty must ever guide us toward right living, and æsthetics is true or false according as it leads toward a higher or lower standard of moral life."[1] Wherever our ideals may lead us, they *lead* us, and we *follow*.

Muscular feeling is (1) immediate, (2) objectively based in muscular contraction, (3) combines diffusion with localization, (4) makes subjective and objective integration (in consciousness) possible, (5) is the origin of the objectively projected idea of Force (the Force-schema), of the subjectively injected idea of Energy,

[1] *Springs of Conduct*, p. 263.

Might (the Energy-schema), of the ejective idea of Power (the Personality schema); (6) it is neuro-muscular—it is involved in and necessary to (a) sensation (localization, intensity, local signs), (b) impulse (outgoing energy), (c) emotion (interacting energy), (d) feeling (pleasure, pain, reaction, satisfaction), (e) relation (subject-object attitude). Yet (for one can dogmatize about opinions), let him be anathema who "evolves" sensation, relation, impulse and emotion "out of" muscular feeling. *An* "emotion" may be a mental attitude involving *all* "elements." I choose to limit the term to effective, reactive, reflex muscular. Muscular feeling ought to be the starting point of psychology and philosophy, because it is the raw stuff of activity throughout the mental sphere, and makes possible an appreciation of the outer world of force. Activity must be guided, and its guidance is all important, but we *start* with the activity itself.

Impulse is the psychological and philosophical standard. Having decided to start with the effective side of the mental life, we shall find ourselves called on to choose a standard of mental currency. Relations may control the flow of our mental money, may adjust supply to demand, may determine amount of reserve fund and kind of investment, but there must be coin of the realm in plenty, and into which mental paper is easily convertible. Shall we choose as the standard, impulse or emotion? Unquestionably impulse. It most nearly represents irritability; it is the effective side of primary instincts; it stands for definiteness, coherence, integration; it is the basis of will and of work. We have needs ("striving impulses"), our active impulses satisfy those needs of objects and of exercise. Emotion is reactive and secondary; its primary function seems to be the reflex overcoming of the inhibition of normal activity. Apart from instinctive outbreaks, emotion in man seems to be acquiring a segregating, recuperative function, and to be more and more characteristic of play as opposed to work. (What we value in an emotion is the intuition, the relation, in it.) Its pathologic tendencies are many and various; emotional characters never become integrated. You can play upon them as on the strings of a harp. Emotions are represented and accompanied in the ejective sphere by pleasure and pain, and are guided by the self-relations. When limited to the function of recuperation, and when recuperation *prepares for* the impulse-attitude-work, emotions are necessary because play is. But impulse is the gold standard of our mental mechanism of exchange. Our wealth is in the unconscious, is organic, and it is our business to invest it productively. Philosophy ought to regard as its normal individuals only those who maintain a sufficient gold reserve fund of spontaneous impulse. Let us have the motor outcomes of innumerable reflex arcs stored up in us as potential energy, to be converted into kinetic volition on the warrants of those controllers of the currency, the relations of right, truth and beauty, as countersigned by the treasurer, our religious ideal. Our play ought to prepare us for work; emotion ought to sustain impulse. We must beware lest our emotional silver become depreciated so that there will be need of a cart-load of sentimentality to equal a gill of kindness.

Pleasure symbolizes attraction, and pain repulsion; objective sensations symbolize activity and the absence of these leads to quiescence. Now sensation (pleasure, pain and six senses) may run a psychological mechanism, but not even in the lower animals do they so. The instinctive impulses and emotions of many animals are full of implicit relation, biological, psychical, social. The empiricists have always made a mistake in stressing pleasure and pain as

"motives to action." Affective sensations diffuse, but in themselves have no effectiveness, no more than objective sensations have. The affective consciousness occupies a small share of normal life as compared with the healthy, almost toneless, work-period. If our potential energy is being replaced as fast as it becomes kinetic, we expect the diffused cœnesthetic emotion to be the result of recreation and therefore to give moderate unobtrusive satisfaction. The great pleasures and the great pains are reflex organic affairs, and will not furnish a basis for philosophy. They are unusual and they have no effectiveness. In extreme cases, the effective sensations paralyze the muscles (become dissociated from effectiveness) and efface consciousness. Nor can philosophy be constructed on the idea that *play* is the goal of life. Play is a preparation for work, for progress, or it has no philosophical meaning. Even the delights of the "æsthetic emotions" are abnormal, harmful, disintegrating, unless they help on that recreation that fits us for work. The rational man would not want to live in a heaven of eternal play. He would ask for the "search for truth," for instance, not to have truth come gurgling down his mental throat. If philosophy, if ethics, wants a *summum bonum* that every normal person can understand, it would not be the "greatest happiness of the greatest number," nor "self-realization," nor "action consonant with law universal," but all of these and more — the most adaptive and expressive work and play that make for the kingdom of heaven. I do not intend to develop this formula, but content myself with adding another ejective (self-objective) maxim: Work for thy living and live for thy work.

The criterion of philosophy is catholic human instinct. When Socrates and his friends had finished their discourses without being able to definitely set forth the essence of courage and wisdom and virtue, he used to say, nevertheless, let us practice them; after Descartes had proved animal automatism satisfactorily to himself, respect for man's relational instincts made him rescue human beings from blank mechanism; when Kant had finished his wonderful feats in the realm of pure reason, he came back to faith and instinct, and exalted spontaneity into a metaphysical principle. The greatest philosophers have been unable to reduce all being to one substance and all relation to one principle because they respected intuition. Some of them have tried to justify the intuitions of God, spontaneity-immortality, some have contented themselves with simple affirmation, some have confessed the impotency of reason to justify them, but all have held to them. The objective lesser lights have given us mechanism, materialism, agnosticism (because we now only "Know in part?"). The subjective logicians have given us mysticism, idealism, pantheism. The lower grade thinkers have set forth eclectic and mixed systems, varying in eclecticism and guess-work. Ejective philosophy, with its object-subject starting point in muscular feeling, and its outward tending work-standard in impulse, strives to investigate the conditions under which truth, beauty and right originate, to set forth its results in empirical terms borrowed from the *objectively* known (*i. e.*, science), seeks its leading principles in the philosophy of character and religion; it believes that there is a purpose in history and in life, that the deepest intuitions of the human race *cannot* be illusions; it believes that philosophy, having had centuries of sight, will return to its primitive attitude of faith. But the content of that attitude is very different. The second childhood of philosophy is not of innocence, but of grace; born of *need* of guiding instincts. Every virile race has its call to the simplicity of nature. But philosophy wants not the simplicity of the savage, but that of earth's

greatest character heroes. Is metaphysic therefore destroyed? I think not. All the sciences have their metaphysic, and philosophy has all the sciences. Skepticism is ruled out, not criticism; materialism and idealism, not physiological psychology and logic, ethics and æsthetics; agnosticism and pantheism, not rationalism and theism. There must be a "synthetic philosophy," but human instinct must judge its results. Above all, do we want a clearing up not only of the "notions" (indeed, some "notions" need critical distinction and others a bodily form), but of the instincts. Ejective philosophy has an effective starting-point and norm, but its criterion is ejective. Of the ejective relations, the æsthetical are the ones that will dominate philosophy; a critique of the æsthetical in nature, in character, in the conceptions of God, immortality and the soul, will, I believe, be a main work of the philosophy of the future. Redintegration, integration, disintegration, habit, law, correspondence with environment, development, character, monism, ideal, type, holiness, fitness, symmetry, harmony—these and many more of like nature are the ideas of the philosophy "that is in the air." Anthropomorphism is becoming æsthetical. As might be expected, theology shows this tendency most plainly: no longer the *Moral* Governor, or the Righteous *Sovereign*, or the *Unknowable* Power; but the Father, who knows and cares about the sparrow's fall, though it must fall; whose sons we are. Religion has ceased to ponder *how* the Father knows or why He cares; it is becoming more and more biological, and, therefore, its ethical life more spantaneous and æsthetical. Men prize spontaneity, but are beginning to say, let the contingent *become* the necessary, let the necessary *be* the spontaneous, and the will on earth correspond to the holiness, the completeness of working of the will in heaven. Absolute right is absolute utility; that all may be will-holy, complete, not that we may be happy, though we will be if we fit in with things eternal. Let *cogito, ergo sum* be *fido ergo fui, spero ergo fero, amo ergo sum.*

Let us, therefore, say that ejective philosophy will exalt æsthetical anthropomorphism, and that it will be biological, instinctive, historical and biblical. Biological: Notice the tremendous sweep of these terms (for natural "analyses" are the parables of philosophy)—cell, differentiatian, degeneration, environment, survival of the fittest, heredity, conformity to type, etc. Instinctive: Critique of instinct. Duty becomes privilege; "the law" becomes "grace" (the instinct of holiness). Historical: Genesis and history of relations. A certain environment is necessary for their *manifestation.* A high brain is a necessary environment for a high character. High brain is a new representation of force just as a high character is a new representation of energy. In character development, integration (kinæsthesis of work and conæsthesis of character, both relational, conceptual), self becomes objectified (an apperceptive eject), a power working for an end set for it. Biblical: Biological, instinctive, historical. Ye work (*final* aim) neither for happiness nor for right, truth and beauty, "but seek ye *first* (primary attitude) *his* kingdom, and *his* righteousness; and all these things shall be added unto you (Christ had just before spoken of the lilies of the field).

At the risk of being tiresome and of repeating, let me sum up the whole matter as it appears to me at this moment. "Prove all things; but hold first to that which is good" (Paul the Apostle: I Thess. v. 21, R. V., marginal reading). Let us see if we have not caught hold of some of the most vital things of faith in the wrong way. Let us not "reconcile" by concessions, by eclecticism, but let us

have a critique of pure relational human instinct, our ideal being the perfect one, who called himself THE *Son of Man*. Let us work, build up, find new philosophical relations, ask for "copy" of science and art and life. Let us have all-sided, energy-based, relation-guided anthropomorphism. Let us take hold of intuitionalist instinct by an empiricist handle. We side with both intuitionalist and empiricist in having philosophy ejective and not effective. Effective philosophy runs into materialism, or into mysticism, or into both, with all grades of abnormality between. Philosophy is representative, and, therefore, it is ejective. Man is not objective (matter) nor subjective (self-feeling), but ejective, self-objective, apperceptive, actively personal, an agent and a steward.

Atheism, agnosticism, pantheism, and the rest of them, are simply tiresome, unæsthetic. They fail to help us to work, they fail to explain things because things cannot be explained either objectively or subjectively, but only ejectively, anthropomorphically, in the higher æsthetic sense of the word. Things are explained when they all fit in together with our purest faith and our best work.

If I succeed in suggesting "what is in the (philosophic) air," I shall be satisfied.

15 East Senate Street.
COLUMBIA, S. C., March 11, 1893.

THE PSYCHOLOGICAL BASIS OF HEGELISM.

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I have already published in the *American Journal of Psychology*[1] what may be considered two contributions towards a new phase of psychological science, viz., the Psychology of Philosophy. As far as I am aware the method of treatment of philosophical systems used in these articles is almost entirely new. It aims at giving the psychological basis of the most fundamental principles of the various systems, and substituting for the mysticisms of the old philosophy more accurate categories obtained by scientific experiment. My first paper offers an examination of the philosophical systems of Hobbes, Locke, Berkeley and Hume from the standpoint of Visualization; in which I attempted to show that many of the chief characteristics and short-comings of these systems have their fundamental basis in the psychological process of Visualization. In the other I have endeavored to point out an important group of conceptions, especially those of Natural Realism, which have their psychological history in the sense of touch. Now I propose to offer a third contribution of the same nature, in which I shall endeavor to trace the influence of the discovery of galvanism in determining the great system of philosophy presented by Hegel.

In the development of the sciences there is perhaps no factor more potent than the psychological process of apperception. The development of the sciences undoubtedly does not take place according to any eternally fixed order, but rather through mutual influences, which are in many cases to a great extent accidental. The various sciences are necessarily interdependent; and out of this direct interdependence which gives rise to reciprocal action between them, there arises an opportunity for the constant activity of a great and powerful influence in determining their development, namely, that process which, since the time of Herbart, is known in psychology by the name of "apperception." The conception gained through the experience of an interesting fact in one department of science serves as an "apperceiving type" for the cognition of newly presented phenomena in other departments. Thus the law of gravitation once observed in a particular phenomenon soon appeared as the fundamental type of all physical action. Everything having the appearance of natural motion or force began to look like one more illustration of the law of gravitation. The discovery of the polarization of light was followed by the apperception of the polarization of heat. The refrangibility observed in light and heat led to the hypothesis of the refrangibility of sound. The theory that evaporation is a solution of water in air was an unconscious assumption that water and air stand to each other in a relation similar to that between salt and water. The theory of the atomic repulsion of the particles of the evaporating fluid, as an ex-

[1] Vol. IV, Nos. 2 and 3.

planation of the diffusion noticed in evaporation, originated by reading into this phenomenon conceptions gained from the observation of electric repulsion. Of such apperceptive knowledge the whole intricate web of scientific theory is full. And this is the particular psychological agency which we must keep constantly in view in tracing the influence of the discovery of galvanism on the mass of scientific and philosophic speculation which led to the production of the Hegelian system.

The phenomenon of attractive and repulsive agency observed in magnetism has long served as a type of scientific and philosophic apperception, and among the ancients as well as among the moderns has often formed the basis of the wildest and most extravagant mysticisms. In the earliest stages of physical and astronomical science all cosmical interactions, all actions between bodies at a distance, could be made intelligible only when classed under the general type of magnetic attraction or repulsion. Everything novel or strange in the shape of attractive or repulsive force, in any department of experience, naturally tended to be apperceived by the mystic mass of ideas gained from observation of magnetic action. So Gilbert tells us, in his "De Magnete," that "the magnet and amber were called in aid by philosophers as illustrations, when our sense is in the dark in abstruse inquiries, and when our reason can go no further." In these earlier times, however, the phenomenon of magnetism stood apart from the great body of intelligible experience as an isolated fact, strange and mysterious in its nature. All facts and all systems of facts, accordingly, apperceived under this type, were on their very face mysticisms of the most mystical calibre. Very different is it, indeed, in the case of that period of discovery and speculation with which we have to deal in tracing that development towards a scientific consensus which followed upon the famous discoveries of Galvani and Volta. In this period magnetic or electric agency still characterizes the "apperceiving mass" dominant in the speculation, but it is no longer an isolated fact: it is now scientific—closely interwoven into, and widely spread throughout, the whole net of scientific experience. In this period we find the electric category of "polarity" presenting itself as the fundamental principle in all the sciences; weaving itself rapidly, through the instrumentality of enthusiastic experimentation, into the basal network of Physics, Chemistry, Mineralogy, Morphology, Anatomy, Physiology, the medical sciences, the social sciences, and various departments in the great body of facts which pertain to general experience. In this period, in short, we find the principle of electricity as it was observed in galvanism accepted as the most satisfactory explanation possible of the fundamental facts of nearly all departments of knowledge; many facts, indeed, which in the light of modern science are as far from finding their explanation in this way as the east is from the west. In this period, consequently, facts apperceived under the type of magnetic or electric agency savored not the least of mysticism, but appeared truly scientific in their inmost fibre.

This difference between electric agency as an apperceptive type among the ancients and as an apperceptive type in the scientific period following the discovery of galvanism, illustrates a very important law of apperception. The one all-important thing which determines to what extent any observed phenomenon shall serve as an apperceiving "mass" for other phenomena is the *interest* which it excites. To the ancients the phenomenon of magnetism was not of universal interest. In fact it was of no *real* interest; it was rather a matter of curiosity. The discovery of galvanism, how-

ever, seemed to carry within it a source of interest most real and intense—an interest that bordered on the wildest excitement. And now it becomes our investigation to inquire into this source of interest. Why all this enthusiasm following the discovery of galvanism? Why should electricity suddenly take such a freak and violently force itself into so many departments of knowledge into which modern science says it had no right to go? What was there about the nature of galvanism that it should excite so much interest?

The great excitement which Galvani's experiments caused all over Europe was due principally to a circumstance in connection with his first discovery, which was purely accidental and unessential to the true value of the science. This circumstance was the intimate connection of the experiments with the animal organism and the phenomena of life. In its first announcement Galvani's discovery was given forth as a manifestation of electricity under a new and remarkable character, namely, as residing in the muscles of animals. The limbs of a dissected frog when touched with two heterogeneous metals were observed to repeat almost all the motions of life. This fact once observed, the first psychological tendency was to secretly suspect—and with great enthusiasm too—the probability that life was identical with electricity. And this once suspected, what could be more attractive than a headlong rush into the investigation of electricity or galvanism? Experiments performed on executed criminals met with such wonderful results that men began to hope that ere long the dead could be raised. Life apparently revived! A thorough knowledge of galvanism will give command over all the forces of life! Inspired by such phantom hopes men waxed enthusiastic and were carried off their feet. A new sun appeared on the scientific horizon in the intoxicated minds of its worshippers. A new sun-myth began to crystallize; a great scientific mythology began to dawn. Introduced in such a spirit of enthusiasm, Galvani's experiments were repeated, with various modifications, in all parts of Europe, exciting the greatest curiosity and giving rise to the most extravagant speculations. The study of galvanism soon became of paramount interest, not only to those actively engaged in scientific investigations, but even to many who were not. Valuable prizes were founded on all sides expressly for promoting its prosecution. Scientific institutions and societies, as well as individual scientists, made it their problem of special research. Commissions from various institutions and societies throughout Europe were appointed to investigate the wonderful results obtained by Galvani and Volta. Not narrow or restricted in its scope, but flashing its magic light into all recesses of scientific research, representatives of all departments of knowledge, Physics, Chemistry, Physiology, Medicine, Biology, Psychology, etc., were found busily engaged in experiment, with a view to its application in their particular subjects of investigation.

The importance of galvanism in the history of science is indeed not less than it was estimated by the great band of enthusiasts who devoted themselves to the speculations to which it gave rise in the first stages of its history; but its permanent scientific value is of a character altogether different from that which suggested itself to their minds. In every great scientific discovery two kinds of value can always be observed. First there is its value as estimated by those who care for it in its childhood—a value which generally passes off into a mass of speculation, and is interesting afterwards only to the psychologist. Secondly, there is its true scientific value into which it finally settles down—a value which remains per-

manent throughout all further development of the science. The real scientific value of the discovery of galvanism consists in none of the speculations to which it at any time gave rise, but in the practical means and many advantages which it ultimately afforded for the observation of a larger number of facts in various departments of science. But in this permanent scientific value we are here not interested; our interest is in its other value—the mystic value attached to it in its infancy. Our interest centres in that apperceptive type of speculation which hovered about all the earlier experiments,—a type of speculation which is well illustrated by a story told of Napoleon Bonaparte just after he had called Volta to Paris. After seeing the decomposition of salts by the voltaic pile, he turned to Corvisart, his physician, and said: "Here, doctor, is the image of life; the vertebral column is the pile, the liver is the negative, the bladder the positive pole."[1] Such speculation as this is deeply interwoven into the inmost fibres of nearly all the thought of scores of the greatest German scientists of the latter part of the eighteenth and the beginning of the nineteenth century.

In its circulation through the various sciences, galvanism had two main avenues of approach. One was the science of chemistry, with which it was essentially connected; the other the science of physiology, with which it was accidently connected.

In the department of chemistry, the decomposition of binary compounds by the "pile" laid the foundation of the whole so-called "electro-chemical" theory. From the wonderful facts collected by numerous experiments along this line, quite early in the history of galvanism, a great theory of modern science was suspected and maintained, viz., the identity of chemical and electrical energy. The chemical *composition* of the fluid with the zinc in the voltaic pile, produces, when the current is completed, a current of electric influences in the wire; and this current, if it pass through an electrolyte, manifests itself there as *decomposition*, overcoming the chemical affinity which resists it. The true scientific bearing of this fact was not properly understood until the time of Faraday. What we want to see, however, is in what way it was understood by his predecessors. That these two sets of phenomena, the electrical and the chemical, were identical in principle, was undoubtedly apprehended and maintained. But in what way was the common principle apperceived? We find that the common principle was still the principle of electricity. Instead of electrical and chemical action being both referred to a common energy which manifests its nature in these two different ways, both were referred to electric agency. The whole process, including chemical and electrical action, was apperceived under the type of electric action. Thus the forces at the point of composition and at the point of decomposition were conceived to be the same force; but this force was looked upon as manifesting itself in the mystical polar opposition of the poles of the magnet or the positive and negative in electricity; composition and decomposition were polar opposites. In the recognition of the intimate relation between the two sciences, the chemical aspect of the supposed common agency underwent severe criticism, but the one thing that remained throughout uncriticised was the electrical aspect. Consequently, it served as the apperceiving "mass" for the facts of chemistry. Thus Berzelius, the great Swedish chemist, and a host of German chemists considered that the descriptions in all

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[1] Becquerel, Traité d'Electricité.

chemical combinations might be "polar." All chemical elements could be considered as electro-positive and electro-negative. All elements were thus classified: Hydrogen, Oxygen, Acid, Alkali, etc. And this "polar opposition" they made the basis of all their chemical doctrines. In the enthusiastic apprehension of this polar relation of the chemical elements, all their phenomenal or sensible qualities were lost to view. The relation of polarity was conceived to constitute the very *essence* of the elements. Hydrogen, for example, was conceived to have its whole being and essence in its polar relation to other elements, just as the north pole of the magnet was conceived to have the essence of its being in its polar opposition to the south. As Oken puts it, "The whole principle or rationale of chemical action consists in the potentiality of two elements to revert to their polar condition." Or, as Hegel puts it, "Objects chemically charged with difference are what they are expressly by that difference alone."

The same tendency of speculation is also exhibited in the science of mineralogy. As the elements of all compounds could be described as polar, that is, could be distinguished as electro-positive and electro-negative, thus giving to every element a place in a series defined by the degree of these relations, the electro-chemical hypothesis seemed to afford a rigorous and complete system of arrangement for the minerals. Accordingly, at one time, we find them arranged according to their electro-positive element, and the elements according to their electro-positive rank; at another time, according to their electro-negative element, and the elements according to their electro-negative rank. Such systems of classification were supported by such men as Berzelius, Gmelin, Bendant and Nordenskiöld; and seemed thoroughly justified by the state of science at the time. Thus in mineralogy, as in chemistry, the tendency of thought which naturally grew out of the exaggerated estimation of the category of "polarity" was to entirely lose sight of the phenomenal or material element. The external properties of minerals, which are the proper object of the study of mineralogy, were made to depend wholly on the electrical relations of their elements. "Such schemes," says Mr. Whewell, in his "History of the Inductive Sciences," "exhibit rather a play of the mere logical faculty, exercising itself on assumed principles, than any attempt at the real interpretation of nature."[1]

From the side of physiology, the investigations were even more enthusiastic. As the experiments were carried on by all the most active scientists of Europe, especially by the Humboldts in Germany, the various phenomena of physiology, one by one, began to exhibit themselves as manifestations of electric agency. In the animal organism, the relation between whole and part was conceived to be that of polar opposition—that mystical "difference in unity" which exhibits itself originally in the phenomena of magnetic and electric action. Muscular contraction was explained as the mutual electric repulsion of the fibres which, on account of their being fastened at the extremities, caused the muscle as a whole to contract. Again the underlying principle of all nervous activity was explained on electrical principles. Alexander von Humboldt made laborious and tedious attempts to explain "nerve currents" and "sympathy" between nerves by electrical "conduction." The animal organism was looked upon as an actual voltaic pile. The solid and fluid parts together formed a galvanic circuit, as did the metals and fluid in Volta's pile. Every irritation, sensation and movement was con-

[1] Vol. III. p. 243.

ceived to be a manifestation of this galvanic agency. Thus Prochaska explains sensation: Any irritating body brought into contact with the organism forms a new link in its galvanic circuit of solid and fluid parts, which causes a quantitative and qualitative change in the electrical tension, which is conducted by the nerves to the brain, and this produces sensation. So also "reflex action." Reflexes, he says, are "founded on electrical attraction and repulsion of advantageous or injurious irritations, according as the *polarities* of the organ and the irritation are identical or opposite," etc., etc.

Into the science of morphology, too, the electrical category worked its way. For example, cells were viewed as standing to one another in the relation of polar opposition. Cell-division was an illustration of electricity differentiating itself into the opposite forms of positive and negative. The accumulation of cells into groups again took place on the principle of electric attraction. By some, the whole process of evolution in the animal kingdom was conceived to be nothing more nor less than a galvanic process exhibited in an endless number of variations. Thus we find such theories as: The male corresponds to the positive and the female to the negative pole. The infusorium is a galvanic point, a galvanic vesicle, a galvanic column or chain.[1] Every change in the process of evolution takes place through the differentialization or the absolution of fixed poles. The number of individual organisms is not persistent; they present the aspect of constant change. They are products of a ceaseless *polarization* or a constant evocation of poles in the great galvanism, positions of the general galvanism in time. As the poles change, so also do the organic individuals. The kingdom of organisms is an iron bar in which the magnetic poles originate and vanish or change, according as the polarizing magnet is removed.[1] Organism itself is galvanism. What would be organic must be galvanic; what would be alive must be galvanic. Galvanism lies at the basis of all organic constitution; it is the rationale of organism. All organic processes are either modifications of it or only its combinations with other and still higher processes, etc., etc.

Not only the partial processes of physiology and the historical changes of morphology were doomed to be explained after this fashion; the same kind of explanation was ardently applied to the higher principles of life and thought. The vital principle was galvanism. We have many illustrations of this doctrine in the history of medicine, especially in the theories of that group of physicians commonly designated as the "natural-philosophical school."[2] John Christian Reil (1759-1813) was probably the first to clearly state the doctrine. According to him, the *vital process* was *galvanism* — a potentialized galvanism. Irritability and sensibility are the two poles. Every organ manifests "polarity." The diaphragm is the indifferent point of the body. "Tension" prevails everywhere between organic and inorganic beings. Death arises from an electric shock by which a neutralization of the "tensions" is accomplished. In this doctrine, he was followed, with more or less variation, by numerous others. Dietrich, G. Rieser (1779-1862), professor at Jena, maintained that "polarity, conceived as a phenomenon, is the basis of everything, since life is understood as an oscillation between a positive and negative pole, and the vital principle as the organic tension which kindles and supports this oscillation." Ignaz Troxler (1790-1866), professor at Berne, conceived the various vital

[1] Oken. Physio-philosophy.
[2] See Bass, History of Medicine.

processes to stand in polar relation to one another: Excretion, secretion, respiration, digestion, etc. Excretion is secretion directed externally, and secretion is excretion directed internally. Respiration and digestion are identical in their essential tendencies and differ only in their relative direction. Life in its inmost character is individual productivity, in which the producing agent and the product are interwoven under the form of self-determination. This idea of self-determination, in which the producing agent and the product are indivisible, is the idea of self-differentiation as manifested in electricity. A similar view was held by Ph. Franz von Walther (1782-1849), professor of surgery, successively at Landshut, Bonn and Munich. He held the true essence of the organism to be that "it admitted no division of the idea of life." The primordial function of life is "self-production," "to which corresponds, in organic nature, magnetism." The original differentiating principle is apperceived under the type of magnetic differentiation. It differentiates itself into the two poles, irritability and sensibility, the former of which corresponds to electrical and the latter to chemical action. This was also the doctrine of Schelling, and of Hegel, too, though perhaps in a disguised form. Among the pupils of Schelling who were mainly physicians, we find the same doctrine carried out to its extremes. All the phenomena and processes of life, and indeed of all nature, were arranged according to "polarities." Everything in nature was polar: Man, woman, irritability, sensibility, subjectivity, objectivity, electricity, magnetism, oxygen, hydrogen, acid, alkali, etc., etc. Here again we see illustrated the fatal tendency of the "polarity-myth." The whole value of empirical facts of all kinds is lost to view, spiritualized away under the mystical metaphor, and the only real thing left in all nature seems to be the relation of polarity. Among those who supported this exaggerated theoretic type of doctrine were also C. H. Pfaff, Humboldt, J. F. Ritter, Rheinholdt, Prochaska, Brandis, Treviranus, Bischoff and Gropengieser.

In the department of psychology the influence of galvanism is well illustrated in the theory of animal magnetism. A great portion of the literature on hypnotism, even up to the present time, is saturated with the mysticism of this doctrine. At the time which we have now under consideration, we find this theory supported by nearly all the physicians already mentioned; also by A. E. Kessler, Wolfart, Prof. Kluge (a special Berlin authority), Hufeland, Eschenmayer, Kieser, Nasse, Walther, Ennemoser, and others.[1] The phenomenon of somnambulism in the hypnotic state was explained thus: The brain with all its dependent organs of motion was regarded as the positive or conscious pole; the sympathetic nerve, with its tissue of ganglia, as the negative or unconscious pole. In the somnambulistic state the vital power is driven from the brain or conscious pole to the sympathetic nerve or unconscious pole, whose larger tissues, especially the "plexus solaris," are turned into a "sensorium," which, as if by substitution, performs the functions of the brain. This was supposed to account for the assertion by somnambulists that their consciousness has its seat in the pit of the stomach. The influence of the magnetizer on the patient was regarded as the action of the outer nervous pole (the brain) of the magnetizer on the corresponding positive pole of the patient, the former repelling the latter, according to the general law of polarization, by which means the nervous power of the magnetizer is concentrated on the nega-

[1] For a full account of these physicians see Bass' History of Medicine.

tive pole of the inner nervous system, the ganglia of the stomach of the patient. Other phases of the same kind of doctrine were dominant with regard to the relation between the brain and metals. Metals were the simplest and most primitive production of the creative force, and consequently were diametrically opposed to the brain. Metal reduces will to its primitive being. The positive or conscious pole in somnambulists is accordingly peculiarly sensitive to contact with metals, etc., etc.

Not only in the special sciences did this galvanic mythology have its sway, it also became deeply rooted in the fundamental constitution of philosophic thought. The tendency of the scientific age is well illustrated by a passage from Schelling. "In the highest perfection of natural science," he says, "the phenomenal or material element must disappear entirely, and only the laws, or formal element, remain. The more law becomes apparent in nature, the more the hull or wrapping disappears; the phenomena themselves become more spiritual, and at last cease altogether. Optical phenomena are nothing more than a system of geometry whose lines are drawn by the light, and the material nature of this light itself is already doubtful. In the phenomena of *magnetism all trace* of *matter* has *already vanished. . . .*"[1] At first sight we might view this tendency as bordering on a genuine idealism, but on closer observation it will be seen that this is not so. It is only a certain conception of matter, matter as a "coarse bulk," and not matter as such that tends to vanish, and what is substituted for it is itself of material origin. In the phenomena of magnetism all trace of matter in the ordinary meaning of the word may have indeed already vanished, but the remaining phenomenon of polarity is not therefore idealistic; it is still material force, and might be called spiritualism or mystic-materialism, but not logical idealism. The illustration which Schelling regards most complete, the illustration of magnetism in which *all trace* of matter has already vanished, shows us clearly the root of this whole tendency to discard "the phenomenal or material element." It is the conception of polarity. Single force or laws of single force could not possibly appear to supply the place of matter; only when force presented itself in the Janus-like form of polarity did it appear able to do this. And only when polarity was read very extensively into the many forces and phenomena of nature did the material element become properly attenuated and appear to vanish. And this is what we find: the conception of polarity being extensively read into the phenomena of nature and the material element as fastly disappearing. There seemed to be polarity in everything. Polarity in the universal law of gravitation, in the form of a ratio of the squares of times to the cubes of distances passed over. Polarity in all mechanical force in its absolute and independent form, namely, the polarity expressed in the union of centripetal and centrifugal force. There is polarity in the colors, the proper objects of optical science. Polarity in the proper objects of mathematical science; for example, in the ratio of the centre to the circumference of the circle. Polarity in more or less explicit form in the proper objects of all the special sciences, inductive and deductive. There is polarity also in the laws of the social sciences. Thus Hegel gives forth a doctrine of social chemism; and the one category of chemism in Hegel's time we saw was polarity. This galvanic chemism is what takes place in the acquisition of a new language. The German "Gauner-

[1] Werke, I, iii, 340 (quoted from A. Seth, Hegelianism and Personality).

sprache" is a chemism of Hebrew and German. The morals and customs of families, states and nations are all outward manifestations of inward social chemism. The same principle is applied to politics, religion and æsthetics. There are polar opposites in politics; for example, anarchism and despotism, each of which when pushed to its extreme, veers round into its opposite. There are polar opposites in religion, the Father and the Son, and the higher unity in the Holy Spirit. There are polar opposites in æsthetics, in tastes; for example, the French romantic and the antique, the neutralization of which two formed the normal taste under Ludwig XIV. Everywhere in nature, everywhere in experience, that two opposed facts could be found standing near to each other, they were conceived to be, now in more, now in less disguised or conscious form, further examples of the electrical category of polarity. The whole world began to appear as nothing more nor less than an infinity of antinomies. Kant in his studies of the four special objects of cosmology recognized what he termed four antinomies of reason. But why leave them in the "realm of shades?" Why any longer look upon them as dead, static, contradictions of reason? Filled with the living power of polarity, they will become four more of those living contradictions which move the world. The antinomies of reason and the polar antinomies of nature are the same antinomies; and the *vitality* of the latter must be read into the former. And they are not four, but spread out *ad infinitum* throughout the rational constitution of the whole universe; they appear in all objects of every kind, in all conceptions, notions and ideas. Every actual thing was thus conceived to involve the coexistence of polar elements. Every element of nature must have its opposite pole, or, as Hegel says, "its own other." The whole world of experience seemed to fall apart into the quaint polarities of Mind-Nature, Subject-Object, Ego-Non-Ego, Thought-Feeling, Sensory-Motor, Active-Passive, Irritability-Sensibility, Male-Female, Day-Night, Good-Evil, Necessity-Contingency, Pleasure-Pain, Vitality-Mortality, Matter-Form, Attraction-Repulsion, Centre-Circumference, Centripetal-Centrifugal, Universal-Singular, Identity-Difference, Organic-Inorganic, Acid-Alkali, Oxygen-Hydrogen, Magnetism-Electricity, Anarchy-Despotism, Father-Son, etc., etc. Polarity everywhere, polarity everything; this was the fundamental category in all knowledge, in science, socialism, politics, religion, appearing sometimes "with open breast" and sometimes in subtle disguise, but always essentially the same category, whether expressed in such forms as the physio-philosophy of Oken or the social chemism of Hegel.

Such, then, was the condition of the scientific age which gave birth to the philosophical system of Hegel. Knowing this, we are not surprised to find Hegel, as he surveys the world about him, philosophizing thus: "Everything that surrounds us may be viewed as an instance of dialectic. We are aware that everything finite, instead of being inflexible and ultimate, is rather changeable and transient; and this is exactly what we mean by that dialectic of the finite, by which the finite, as implicitly other than what it is, is forced to surrender its own immediate or natural being, *and to turn suddenly into its opposite*."[1] "Everything is opposite. Neither in heaven nor in earth, neither in the world of mind nor of nature, is there anywhere such an abstract, 'either—or' as the understanding maintains."[2] "Wherever there is movement, wherever there is

[1] Wallace's Hegel's Logic, p. 128.

[2] *Ibid.*, p. 192.

life, wherever anything is carried into effect in the actual world, there dialectic is at work. It is also the soul of all knowledge that is truly scientific." "Every abstract form of the understanding, taken precisely as it is given, naturally veers round into its opposite."[1] "Contradiction, above all things, is what moves the world, and it is ridiculous to say that contradiction is unthinkable."

Let us now turn our attention to Hegel's logic with a view to determining what part the category of polarity played in the formation of his system.

As we go on with this investigation let the object of our search be well defined. We recognize in Hegel the laborious and comprehensive student, the patient observer of facts. We recognize in his work the value of the vast number of important facts which pass before us in the "march of the object" towards its completion in the motion. We recognize the profound philosophical criticism of previous systems which accompanies nearly every step of the dialectical evolution. We can agree with many of the general conclusions. But with all these things we have nothing to do. What we must confine ourselves to is the Hegelian philosophy in so far as it is a distinct system. Our question is not concerned with the value of the facts observed and collected, but with Hegel's peculiar interpretation of these facts. It is concerned especially as to the nature of the one fundamental principle of the system. What is it? And with what aptness has it been applied to the facts of experience? Hegel is dissatisfied with the limited problem of the *Erkenntnisstheorie* of Kant as to whether the categories are subjective or objective; he is tired of the bare formalism of the Fichtean idealism; and is indignant over the abstract identity of Schelling. With firm faith in the objective validity of reason and strong determination to rid himself entirely of abstract formalism, he plunges anew into the world of experience, grasps his philosophical principle from the essential constitution of objective fact, and rests not until he has followed its evolution into its most concrete details. Such is his profession. Granted there is a sense in which all this is true, our question still maintains its validity. It now formulates itself thus: Has the principle been grasped from the absolute and eternal nature of facts, or has it been taken only from the nature of facts as apperceived under the most interesting scientific category of the age? Is it a principle of *pure* thought or is it founded in experience? Is it eternally true, or has it a psychological basis and history? Or, to state the question more definitely, how far is it a principle of pure thought and how far is it determined by that galvanic mysticism in the midst of which it was evolved?

By making a general survey of the procedure in the logic, one can readily observe that the method and phraseology used are preëminently like those used in the science of galvanism. The one principle of movement which is over and over again repeated throughout the whole system is that of the affirmation, negation, unity of the two; or positive, negative, indifference-point. The process begins with being. Being is positive; nought is its negative aspect; the result is becoming. Then the same process begins again. Becoming has a positive aspect—an aspect of immediacy—it is being-determinate. Being-determinate begins the process as "somewhat," which is positive; the negative is "other;" the new result is being-for-self. And so the evolution goes on, the same triple movement being repeated again and again, with no new

[1] *Ibid.*, p. 127.

element save that contained in the variety of facts which present themselves for arrangement.

But this consideration is external. Hegel might be thus indebted to electric science for his peculiar phraseology, and yet have obtained his fundamental principle from an entirely different and supremely higher source. Accordingly, the evidence which is to have validity in determining the question must be internal. What we must do, therefore, is to make an internal criticism of the logic.

If there is one place more than another in all the logic where the *nerve* of Hegel's philosophical principle is laid bare, it is in the doctrine of essence. In this, we find the essential foundation of the dialectic made explicit. And the nerve of the doctrine of essence is the relation of identity and difference. These two categories standing together in a certain peculiar relation, form the basis of the whole logical procedure. In every step of the evolution, we find them actively present. In the doctrine of essence, Hegel, for the first time, makes explicit what he means by them, and what particular kind of relation he conceives to exist between them. If we learn this well, then, we practically learn all.

Hegel begins his account of these categories with a statement of some true and very important facts concerning them. The position adhered to in the "formal logic" with regard to them is stated and criticised. The inconsistency and untenability of such notions as "abstract identity" and "abstract difference" or "mere variety" are clearly set forth. The indivisibility of identity and difference is insisted upon. Whenever we reflect on the notion of identity, we see that it implies difference; whenever we reflect on the meaning of difference, we find that it implies identity, etc., etc. But in all these criticisms and statements of facts, he is not yet stating his principle of interpretation; he is only preparing the way for it. Now let us see how he states his interpretation. He begins thus: "Difference implicit or in itself is a difference of the essence, and includes both the *positive* and *negative*, and that in this way: The positive is the identical connection of self in such a way as not to be negative, and the negative is the different by itself so as not to be positive. Thus either is on its own account, in proportion as it is not the other. The one shows in the other, and is only in so far as that other is."[1] So far anyone will readily observe that what is stated is the theory of the relation between positive and negative in electricity. But he continues: "The essential difference is, therefore, opposition; according to which the different is not faced by *any* other but by *its own* other or special antithesis."[1] This last passage gives us the key to the whole matter. If we fully grasp all that is implied in this peculiar kind of "opposition," in which the different is not faced by *any* other but by *its own* other, it will at once appear clear that the principle cannot be one of pure logic or pure thought. Professor A. Seth, in his work entitled "Hegelianism and Personality," says: "The opposition which Hegel makes his fulcrum is contrary or real opposition; the second is not simply the negative of the first, but both are real determinations of things. But if this is so, then the first does not of itself strike round into its opposite. The opposite arises for a subjective reflection which has the advantage of acquaintance with the real world." This is undoubtedly true. The dialectical evolution cannot possibly be a process of pure thought. The opposition is *real;* the two elements are *real* determinations of *things*, and consequently must be learned through experience. But I think there is still more definiteness implied in Hegel's opposition.

[1] Wallace's Hegel's Logic, p. 189.

It is restricted not only to *real* opposition, but it is a *particular kind* of real opposition. Each element has *its own other*, its own special antithesis. Now this is not necessarily even a matter of experience —that is, of necessary experience. It would require a *particular* experience to know the special opposition of things. The opposition is not of the nature of that opposition with which we are acquainted in general experience—it is a very peculiar kind of opposition. Now what is it? How does Hegel define it? The best definition he can find is the conception of polarity in physics—not the best *illustration*, but the best *definition*. "The conception of polarity," he says, "which is so dominant in physics, contains by implication the more correct definition of opposition."[1]

Let us stop a moment and reflect. As we read Hegel's description of his fundamental principle in this chapter on essence, and watch him drawing it out, as he professes, from the rational constitution of things, can we observe whence he is taking it? Is he taking it from logic or from physics? It certainly does not savor of logic. If it is logic, it is certainly an entirely novel kind of logic. Nothing is more detestable in Hegel's sight than the "ordinary logic." He takes its matter as his matter, but gives it an entirely new setting. Now what does this new setting amount to? What is he really doing? Is he not just reading into logic the "polarity science" which was so dominant at his time? The conception of polarity had been well spread through the physical sciences by others. Hegel recognizes this. "In modern physical science," he says, "the opposition first observed to exist in magnetism as polarity, has come to be regarded as a universal law pervading the whole of nature."[2] And he approves of this as "a genuine advance in science." But no one as yet had applied the conception to the sphere of thought. Physicists, when they had any speculation to do in this sphere, still adhered to the old formal logic. And Hegel contemptuously disapproves of this. After urging the conception of "polarity" as the more correct definition of that "opposition" which he makes the fulcrum of his dialectic, he finds fault with the physical scientists thus: "But physics, when it has to deal with thoughts, adheres to the ordinary logic; and it may, therefore, well be horrified in case it should ever expand the conception of polarity, and see the thoughts which are implied in it."[3]

I do not mean to say that Hegel is consciously trying to apply the physical category of polarity to logic. He is not trying to give a physical interpretation of thought. His aim and procedure are far above such type of materialism. What he means to do is rather to apply the principle implied in the physical conception to the constitution of thought. But how much of what is implied in "polarity" does he show us? Simply nothing. The one principle or conception which remains with him wholly uncriticised throughout is the conception of polarity. The very kind of polar behavior which we perceive in physical phenomena is exactly the same behavior which we observe in the dialectic. Whenever Hegel reaches this peculiar kind of behavior, he is satisfied; he has no desire to go any further. He never asks whether this behavior is itself rational. As a rational principle, it is as unintelligible in Hegel's applications of it as it is in the phenomena of electricity. Consequently, it is the bare physical conception which forms the type of thought under which he apperceives the matter of logic.

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[1] Wallace's Hegel's Logic, p. 191.
[2] Wallace's Hegel's Logic, p. 192.
[3] Wallace's Hegel's Logic, p. 191.

This conception of polarity we find at the heart of all the most important definitions of his principle. "Positive and negative," he continues, "are, therefore, intrinsically conditioned by one another, and have a being only when they are connectively referred to each other. The north pole of the magnet cannot be without the south pole, and *vice versa*. If we cut a magnet in two, we have not a north pole in one piece and a south pole in another. Similarly, in electricity, the positive and the negative are not two diverse and independent fluids. In opposition, the different is not followed by any other, but by its own other. Usually we regard different things as unaffected by each other. Thus we say: I am a human being, and around me are air, water, animals, and all sorts of things. Everything is thus put outside of every other. But the aim of philosophy is to banish indifference and to learn the necessity of things. By that means, the other is seen to stand over against its other. Thus, for example, inorganic nature is not to be considered merely something else than organic nature, but the necessary antithesis of it."[1]

The fundamental laws in electrical phenomena are the repulsion of the homogeneous and the attraction of the heterogeneous, which are in reality only two phases of the same law. In this law, Hegel sees the unity of attraction and repulsion. Thus the positive and negative, in so far as they are positive and negative, are heterogeneous, and already involve *repulsion*. But the positive and negative are always attracting each other. Hence, we never find repulsion without attraction. So, conversely, we never find attraction without repulsion. Homogeneity means abstract attraction—that is, attraction as it would exist without repulsion. But the homogeneous always repel; hence, attraction involves repulsion. Now Hegel treats this law as a universal principle of reason, and it is the one law on which he falls back for all his explanations. In this way, for example, he explains infinity. If we look upon the relation between "somewhat" and "other" as mere repulsion—as heterogeneous, we get an endless progression—a bastard infinity. But if we remember in this that the *unlike* always attract, we get the true infinity. The true infinite is not merely the opposition of the "somewhat" and "other," but the unity or attraction of them in their opposition — the unity of attraction and repulsion. In all this, we can readily see that Hegel is not proceeding from a logical source, but rather reading into the laws of logic the law of electric action. The so-called law of attraction and repulsion is not a universal law of reason as Hegel maintains; it is peculiar to the phenomena of electricity. Homogeneity is not absolute attraction nor is it like attraction; neither is heterogeneity repulsion.

Another consideration, closely connected with this, substantiates this view. Hegel's categories are all living, active things; they seem to be quickened with physical force. As Professor Seth says, they take upon themselves flesh and blood and actually walk into the air. Each finite category *induces* its opposite as positive induces negative electricity. Thus the heterogeneity in the "one and the many" is actual repulsion. "The one manifests an utter incompatibility with self, a self-repulsion; and what it makes itself explicitly *be* is the many." So the opposition between "somewhat" and "other" is not merely logical distinction, but real physical change; it is the real alteration or mutability of the physical world[2], etc., etc. Some explain this by saying that the chief characteristic

[1] Wallace's Hegel's Logic, p. 191.
[2] Wallace's Hegel's Logic, p. 149.

of Hegel's system is that it is the unity of logic and metaphysics — a logic that is at the same time a metaphysic, and claim this as "the beauty" of the system. But the facts already noted lead us to believe differently. Hegel's logic is not the unity of logic and metaphysics; it is rather the unity of logic and speculative physics; and in this respect, there is no "beauty" in it.

Granted that Hegel got the first suggestion of his philosophical principle in the conception of polarity, it may still be maintained that he rose supremely above it, and latterly used it only as an illustration. A position similar to this is held by Mr. W. T. Harris. "The language which Hegel uses," says Mr. Harris, "shows the road over which he traveled to the thought of this self-active essence presupposed by all phenomena. It indicates his studies of Schelling and his predecessors, Kant and Fichte. Hence, too, his illustration of his thoughts. He calls up the law of universal gravitation as the very notion itself of law as lying behind the play of forces. It is that which constitutes its great significance, he tells us. So, too, electricity, which as simple power manifests itself as self-opposition or polarity of positive and negative. Gravitation, too, has polarization or duality, taking the form of time and space relations, the ratio of the squares of times to the cubes of distances passed over. We can see how Schelling's symbol of polarity and the point of indifference are the original subject of Hegel's investigation here, and that he thought it out in this universal form, changing a symbol derived from a mere particular object, a magnet, into general abstract thoughts—pure thoughts."[1] The process by which the empirical conception is changed into *pure* thought is illustrated thus: "The magnet, for example, was a brilliant metaphor and stimulated reflection at first. But owing to its peculiar limitations, which made it only a magnet and not the World-Spirit, it soon began to mislead. . . . . . For the magnet's poles are mere north and south directions, and not subject and object as in consciousness."[2]

We can fully agree with Mr. Harris until he brings us to the process of transforming empirical into *pure* thoughts. The wonderful metamorphosis takes place upon transferring the thought from the magnet to the World-Spirit. Let us not be carried away by the sublime transition. Such a transition avails us nothing. It does not make my conception of atoms one whit purer to say that atoms constitute the essence of the World-Spirit; it only makes it more mystical. Certainly an apperceiving thought is often transformed and enriched through its application to new facts. This is a law of apperception. For example, in the history of science it often happens that some discovery, at the same time it is apperceived, transforms the whole system of our knowledge. No better illustration of this can be found, perhaps, than the discovery of galvanism, which we have already considered. But this psychological fact must be distinguished from that for which Mr. Harris contends. We find that all such transformations and enrichments proceed from the side of experience; that the new observation in order to be enriching must be an actual and interesting *sense* experience; and that apperceiving ideas are very slightly enriched in their application to speculative objects, but rather are made mystical. Consequently, no difference what object, and however sublime it may be, to which an empirical thought may be applied, it can never be changed into a *pure* thought in the Hegelian sense of the term.

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[1] Hegel's Logic, p. 70 (Grigg's Phil. Cla., 1890).
[2] *Ibid.*, p. 71.

Another possible interpretation of Mr. Harris' view is this. The thought is pure from the first. The experience of the phenomena of magnetism serves as the occasion for its first imperfect realization. The transition to the World-Spirit makes it at home with itself and reveals it in its purity. The pure thought is the thought of "self-opposition." But to this we may answer: The principle of self-opposition is not a pure thought, it is only a postulate—an empirical postulate. It has been from the first a postulate of physical science just as the atomic theory is a postulate of physical science; it is of great value as a postulate just as the atomic theory is valuable, but as a *real* principle of pure thought or pure magnetism, or pure anything else, it is as unintelligible as the infinitely infinite number of atoms which would be necessary to the constitution of the physical world.

Schelling's philosophy is based on the symbol of the magnet. But Hegel's principle is an advance over that of Schelling, and this, Mr. Harris from another point of view attempts to show, is a step taken from the physical symbol to pure thoughts. Schelling's absolute corresponds to the "indifference point" of the magnet. Now this indifference point is wholly devoid of polarity, a mere indifference utterly indeterminate—a sort of zero or nothing. The one pole is mind and the other nature, and the absolute essence is the point of indifference, a substance that is neither mind nor matter. This absolute accordingly transcends not only matter, but also intelligence; it is a supreme unity utterly devoid of determination; it is, as Hegel says, no better than the night, in which all cows are black. Now Hegel's conception of the absolute is very different. Schelling laid all stress on the indifference point or identity of the two poles. Hegel conceives that this is important, but that it is not all; the polar opposition or difference is equally as important. Schelling's absolute cannot be called the creator, "for to create is to impart substance and existence, and such impartation would be self-separation and not 'indifference,' but rather a polar difference of positive and negative, or active and passive within itself."[1] This emphasis of difference or polar opposition, then, is the advance made by Hegel. In this, however, he is not at all transcending the symbol of magnetism. He is only presenting a more complete view of magnetic phenomena. He grasps the magnet as a totality. The all-important aspect of it is not the indifference-point: the opposition of the poles is equally important. These two phases of agency always stand together in the conception of the magnet as a totality, namely, the identity of the two poles in the indifference point and their opposition at the poles themselves. Thus we have the supreme law of Hegelism: the unity of attraction and repulsion—the unity of identity and difference without the destruction of either.

The philosophy of Schelling is characteristically distinguished as the philosophy of identity. That of Hegel may be characterized as the philosophy of the syllogism. The syllogism, however, taken "not as it was understood in the old formal logic, but at its real value," in which "it gives expression to the law that every particular thing is a middle term which *fuses* together the extremes of the universal and the singular."[1] The conceptions of both philosophers are based on the conception of magnetism, and the difference between the conceptions is due to the difference in their views of the magnet. For Schelling the magnet was Identity, for Hegel it was the Syllogism.[2]

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[1] Harris' Hegel's Logic, p. 71.
[2] Wallace's Hegel's Logic, pp. 41, 42.

What we have accomplished thus far is a consideration of the Hegelian principle as such. We have considered some of the most important points of the definition and illustration which are calculated to give the most direct evidence as to the nature of that peculiar kind of relation which Hegel conceived to contribute the rational essence of things. The New-Hegelians follow him in the general conclusion that the essential nature of the world consists in "relations." But this position is much more general and indefinite than that of the master. His, as we have seen, is a peculiar kind of relation. And from his definitions, descriptions and illustrations of it, our only conclusion is that it is the relation of polarity as presented in the physical phenomena of electricity. Let this suffice, then, for direct evidence; we shall now consider some points of indirect evidence. We shall consider briefly the principle in its application to the problems of philosophy, with a view to determining how far this psychological interpretation will account for some of the chief difficulties and short-comings of the Hegelian philosophy.

Let us first consider the dialectic as an evolution of thought. Is it a purely sympathetic process? or is it after all an empirical process? This is a dispute of long standing among the critics of Hegel. Trendelenburg and his followers maintain that the procedure of the evolution is not an original synthesis, but a sort of empirical synthesis which is the result of a previous analysis or abstraction. All the elements of thoughts, according to this school of critics, in their original form are intimately united in the concrete forms of experience. By abstraction these elements are violently held apart. What is thus violated by abstraction, however, cannot but strive to escape from this forced position; it must strive to complete itself. When this completion takes place there will arise a new conception which contains the former in itself. This new conception, again, will repeat the process; and so the evolution will go on until the full reality and concreteness of perception have been restored. Thus, for example, "if Becoming is clear to us through perception, there may easily be distinguished in it the moments of Being and Non-Being. Thus while day is dawning, we may say 'it is already day,' and also 'it is not day.' We separate and distinguish these moments in Becoming as actually observed, *but without in the least understanding logically* the characteristic of real existence in virtue of which they are present together."[1] The motionless ideas of Pure Being and Pure Nothing could never of themselves give rise to the movement of Becoming unless the idea of becoming were presupposed. The synthesis in the movement, then, is not a true synthesis; it is due to previous experience; a retracing of our steps from the concrete to the abstract. Such is the view of the Trendelenburg school. But perhaps a larger number of critics maintain, on the other hand, that the synthesis is a real one, pure, original. And indeed this view corresponds more faithfully with the profession of Hegel. The open pretence of the dialectic is that it is an entirely presupposition movement; a real evolution of one category out of another. The criticism contained in the dialectic is not the criticism of an external subjective reflection, but an immanent criticism of one category by another; the march of the object itself. And so the discussion goes on. There seems to be a good deal of truth on both sides of the question. From the purely philosophical standpoint the question is a source of real perplexity. Only one of these views can be philosophically true; yet in the Hegelian system both are clearly observable. Now how are we to account for this difficulty?

[1] Trendelenburg, "Logische Untersuchungen," 1, 38.

According to the psychological view for which we are contending, there are two lines of thought which must run parallel throughout Hegel's system: first, there is the series of facts which he brings up for explanation, and secondly, the principle which he reads into these facts. The real facts of thought-evolution which he observes and endeavors to explain, are the facts noted in the view of the Trendelenburg critics; the living synthesis implied in the attraction and repulsion of electric agency is the pure and original synthesis noted by the opposing school of critics.

That the real synthesis apparent in the Dialectic is just the conception of electric synthesis and nothing more is evident from many considerations. It is clearly implied in many facts we have already noted. We have already seen, for example, that the categories are living things, expressing themselves in such forms as physical mutability, etc. And when a category brings about its opposite in the dialectic, it does so not by the power of a mere logical *distinction*, but it does so actually and really, as if by electric *induction*. Again when Hegel speaks of the sciences of magnetism, electricity and chemistry in the Naturphilosophie, what does he call them? "The dialectical sciences." He calls the principle of these sciences "dialectic." Hegel's descriptions of the synthetic process are full of such evidences. In all his descriptions of the synthesis one can see no description which will not apply to electrical phenomena. What, then, are we to say? Our answer must be that, in so far as Hegel tells us what the synthetic principle *is*, we are to understand by it nothing more than the principle of electricity. But the advocate for pure synthesis has one more chance. If he has failed to convince us by telling us what the principle *is*, he may accomplish his project by showing us what it *does*. If the principle is one of rational synthesis it must show itself as a source of real development; it must actually develop into the various stages presented in the dialectic. If it can do this we must admit without further discussion that it is a real principle of reason above and beyond the descriptions of it which we have already observed.

But can it do this? A careful observation of the procedure in the Logic will convince any unbiased observer that in this also it fails. The real advances made in the evolution all seem to be made possible only by reference to experience; they belong to the kind of procedure noted by the Trendelenburg critics. Synthetic process, it will be noticed, is always at a standstill; it never develops into anything, but is ever the same old process repeated over and over again. Take, for example, the section of the evolution beginning at identity. Let us suppose that we understand the synthetic process by which identity and difference coalesce in the "ground." Now why does the evolution not stop here? What is the motor power that carries the process out of the ground? Let Hegel answer. "We must be careful, when we say that the ground is the unity of identity and difference, not to understand an abstract identity. Otherwise we only change the name, while we still think the identity of understanding which has been already proved to be false. To avoid this misconception we may say the ground, besides being the unity, is also the difference of identity and difference. The ground, which originally seemed to supersede and swallow up contradiction, thus presents to us a new contradiction."[1] We see by this that the synthetic process which is to take place in the next triad after mediation of identity and difference in the ground, is gotten by a bald repetition of the old process;

[1] Wallace's Hegel's Logic, p. 193.

the ground, which is a category taken from experience in order to be made synthetic, is apperceived as "the *identity* and *difference* of identity and difference." In this we can observe the true method of Hegel's artful procedure. The synthetic principle itself does not develop, but is always static, always the same; and the secret of its activity seems to be that it is repeatedly read into the development that really does take place, namely, the development due to empirical association. This is the method throughout the whole system. Hegel never tires of warning us that such phrases as "being and nothing are the same," or "the unity of being and nothing," and all other such unities, that of subject and object, and others, misrepresent the facts by giving an exclusive prominence to the unity, and leaving the difference which undoubtedly exists in it without any express mention or notice. The immediacy or unity expressed in the third category in each triad is thus only a farce as far as the synthetic evolution is concerned; what must be done in order to get a synthetic movement in each new triad is to repeat the old process. Thus we observe that the so-called synthetic principle has nothing whatever to do with the real development; it does not develop at all, but is only repeated. Hegel, however, so artfully mixes up this repeated synthesis with the steps of the real evolution, that it is very difficult to distinguish them. Hence the double aspect of the dialectic. The substance of our conclusion, then, is this: The evolution of thought in so far as Hegel presents it as synthetic is an evolution that never advances a step, an evolution which is nothing more than the repetition in each tried of the category of polarity; and the development which really does take place is the movement from abstraction to the concrete forms of experience.

The real principle of development in the dialectic, then, is the psychological principle implied in the empirical facts brought up for explanation, and not the principle which Hegel endeavors to read into these facts. And this, we shall find, is the really valuable thing and centre of attraction throughout the whole system, namely, the empirical facts observed, and not the interpretation given. What is, we ask, the great centre of attraction in the Hegelian Logic? Probably his treatment and application of the central categories of identity and difference. Now what is so attractive in his discussion of these categories? Undoubtedly it is some great truth which lies hidden somewhere in his theory of the essential relativity of thought. "All knowledge consists in relations," say the New-Hegelians. "The whole world is essentially made up of thought relations." This is the attractive point. But what is the great truth we find beneath this doctrine of "thought-relations?" It is that the laws of the relativity of thought are ultimately not logical laws at all, but physiological. This is a great fact proved beyond reasonable doubt by experiment, namely, that the so-called "relativity" is ultimately not the relativity of consciousness, but the relativity of sensations. And what makes Hegel's treatment of identity and difference so attractive is that it contains within it a comparatively accurate statement of the laws of discriminative sensibility. In my paper on Natural Realism I endeavored to show that the facts which the realists aimed at expressing were the laws of tactual perception, and that in these ultimate facts of tactile sensibility lay the attractive centre of their doctrine. So here the facts which Hegel is striving to express in this discussion are the physiological laws of discriminative sensibility, and in this lies the really valuable and attractive element of his doctrine. And so in the case of all the categories: the valuable and attractive thing is

not the interpretation given, but the statement of the facts themselves.

We have examined the principle as a principle of development: now let us examine it as a principle of explanation. In this capacity also, we shall find that it does not vindicate its right to be called a principle of universal reason. We said before that the one conception which remains wholly uncriticised with Hegel is the conception of polarity. Now we shall find that his uncritical application of this conception to the nature of reason is the chief source of his errors in philosophical explanation. The powerful opposition which moves the world does not prove itself rational. Its insufficiency as a rational principle presents itself in its root form in the highest stage of the dialectical opposition, the opposition between mind and nature. Nature is the "other" of mind—its special antithesis. We are told that nature is implicitly mind, and therefore ultimately rational. Let us admit that we are satisfied with this half of the interpretation. But there is the other half which is equally important. Nature is the "other" or special antithesis of mind, and in this respect it is irrational. Now how is this phase of nature explained? Hegel occasionally calls it "illusion," but his more explicit statements of the doctrine clearly show that "illusion" does not at all express what he means. And indeed we can readily see the truth of this if we remember that "illusion" is a category which is applicable only to psychological phenomena and not to the nature of reason. The "other," according to Hegel's genuine doctrine, must "have its scope;" it is a real, necessary thing in so far as it is "other," and must not be explained away. It is, in short, a real and definite phase of reason. Now here is the difficulty. The "other" is a real, definite, extensive irrationality which we must not attempt to rationalize, and yet Hegel insists that it is ultimately rational. This difficulty is not merely a little hole that can be picked in the system just at this point: it is a huge insufficiency that pervades all Hegelism, and can be observed in more or less explicit form in every stage of the dialectic. Let us look, for example, at the transition from teleology, which is the last stage of objectivity, to the idea. The finitude of the end or aim in teleology consists in the circumstance that, in the process of realizing it, the material which is employed as a means, is only externally subsumed under it and made conformable to it. The transition to the idea is explained thus: "But, as a matter of fact, the object is the notion implicitly; and thus when the notion, in the shape of end or aim, is realized in the object, we have but the manifestation of the inner nature of the object itself. Objectivity is thus only a shell or covering under which the notion lies concealed." Let us for a time admit the "matter of fact" that the object is the notion implicitly: but what about the opposition which has just been annulled? This is the essential point to be explained, and the only account we get of it at this time is that it is "a shell or covering under which the notion lies concealed." We may understand what is meant by the shell or covering of an oyster or a chestnut, but the shell or covering of reason is a metaphor which means nothing. But the curious thing about it all is that we are not supposed to understand the so-called "shell." It must have its scope as such, and the true explanation of it is that it is inexplicable. While the scope of objectivity is being emphasized, this is the view adhered to, but when the transition to the idea is made, it is subtly abandoned, and the view of ultimate rationality is put in its place. The two views are never harmonized, but each is sacrificed to the other according as objectivity or the idea is emphasized.

Take, as another illustration, the doctrine of contingency. Though "the contingent is only one side of the actual," yet as such it "has no less than the rest of the forms of the idea, its due office in the world of objects." "On the surface of nature, so to speak, chance ranges unchecked, and that contingency must simply be recognized, without the pretension which is sometimes, but erroneously, ascribed to philosophy, as seeking in it a necessary and rigidly fixed law. Nor is contingency less visible in the world of mind. The will, as we have already remarked, involves contingency under the shape of option or free-choice, but involves it only as a vanishing and abrogated element. In respect of mind and its effects, just as in the case of nature, we must guard against being misled by a well meant endeavor after rational knowledge, which would fain exhibit the necessity of phenomena which are marked by a decided contingency, and try, as the phrase is, to construe them *a priori*."[1]

In this we see that chance ranges unchecked and must simply be recognized as such. The only explanation that can be given is simply to recognize that it exists in the world, and is in its essential nature irrational and inexplicable. The contingent, however, is only "one side of the actual." The other side is rational necessity, and the side of contingency when reviewed in its union with this, Hegel conceives is some way or other brought into a rational system. But in whatever way we may conceive this to be done we must be careful not to explain away the fact of contingency; it still must have its scope. Now the only possible way on these conditions to bring contingency within a rational system is to "lump it" and tie it mechanically to reason. But this is a mere jumble of words and contradiction of terms. By contingency we mean that we can give no rational account of why things are as they are and not otherwise. And to let contingency range unchecked without seeking in it rational knowledge, to leave it eternally as it is on the surface, and yet attempt to bring it within a rational system, amounts to saying that in the highest stage of reason we can have rational irrationalities. Thus the higher unity of reason in which Hegel harmonizes necessity and contingency is a reason in which anything may be anything else.

The same transparent fallacy may be again illustrated in the doctrine of identity and difference. The "other" of the rational element in this case is "mere variety." Mere variety by itself is untenable, but as the "other" it must have "its scope." It is the most common thing in the whole world. "Usually we regard different things as unaffected by each other. Thus we say: I am a human being, and around me are air, water, animals, and all sorts of things. Everything is thus put outside of every other."[2] The other side of the doctrine is expressed in the following statement that "the aim of philosophy is to banish indifference and to learn the necessity of things." But this aim of philosophy, according to Hegel's rendering of it, never reaches the inner fibres of the nature of variety—it only regards it externally. It never accounts for the fact of variety. It does not explain it, or even attempt to explain it: it rather regards it as a weak and unimportant thing and treats it with contempt.

We might go on with illustrations, but it seems unnecessary. The same fallacy appears in all the various forms which the central opposition between mind and nature assumes. Nature as the "unaufgelösten Widerspruch" always remains a huge lump of matter, foreign to reason. Hegel seems to maintain that as such it is inex-

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[1] Wallace's Hegel's Logic, p. 228.
[2] Wallace's Hegel's Logic, p. 191.

plicable, and that the true way to explain it is to ignore it. He does give it a kind of explanation, but not a philosophical explanation. It is a sort of wholesale explanation, an explanation which does not penetrate into the inner network of nature, but only gives it an external and mechanical connection in the rational system. The fundamental insufficiency of this explanation is very artfully concealed beneath the veil of the familiar category "immediacy." Immediacy serves as a great box into which he casts all the irrationalities of the world. He then closes up the box, calls it a moment in rational conciousness, and declares that by so doing he has rationalized all.

But Hegel will make one more attempt to overcome the difficulty. He will insist that the opposition between reason and nature is all the time within the bounds of reason. Thought overlaps nature, the subjective overlaps the objective; the former is always wealthier than the latter. And by means of this overlapping of the rational, the irrational is ultimately some way or other made rational. Reason is itself essentially a triple movement, embracing within its necessary activity both the opposition between itself and nature and their ultimate unification. The two movements are only two phases of the one essential activity of reason. Now this view does for a moment seem to lift us out of the difficulty. It is undoubtedly very attractive, and on the surface quite satisfactory. But on closer observation one will observe that such a doctrine holds good only when reason is regarded in the abstract. In all this talk about reason, we never bring into realization what we mean by it. We are, in all probability, thinking of something which has in common with reason very little more than the name. We might, with equal intelligence, talk of anything going through the same movements, for example, electricity. We have seen already that when Hegel regards reason in its actuality, this principle of triple movement is not applicable to it. He fails to read this abstract triple movement in the manifestations of reason in the real world, such as subjectivity and objectivity, identity and variety, necessity and contingency. If we regard reason in the concrete and try to apply this abstract principle to it, we shall find that we are necessarily led into one of two doctrines neither of which satisfies the end which Hegel claims to have accomplished. In the first place the application may mean that the opposition between mind and nature is ultimately wholly done away with; that in the fully realized idea it does not exist. But this would amount to a mere formal subjective type of idealism in which the whole value of objectivity would be lost, an idealism which would be ignored by Hegel. In the second place it may mean that the rigidity of the opposition is always maintained; that it is an eternal necessity of reason. This is undoubtedly Hegel's meaning. But the opposition is at the same time harmonized. Now what must be the nature of that higher unity which makes this harmony possible? It cannot be called reason—that is, the reason which is opposed to the irrational—for if that were the case the harmony of the opposition could mean nothing else than its complete destruction. Nature, we must remember, is opposed to the absolute and complete nature of reason; it is its special antithesis: the opposition is between rational and irrational. Consequently the one may conquer the other and thus bring about harmony, but in order to retain the opposition and yet become harmonized a third party is necessary in which they must receive this new relation. Thus, in this case, the principle of unity of the rational and irrational which Hegel still

calls reason, is in reality a principle which transcends both mind and nature, a principle identical with the absolute of Schelling.

The Hegelian principle, then, whatever it may be, is clearly not an ultimate principle of reason. Reason bends under its power. Rationality and irrationality are alike moments in it. The opposition which it makes between mind and nature is an opposition which transcends rationality, and any attempt to bring it within a rational system lands us either in subjective idealism or in a doctrine of blank identity. This is the case when we keep reason before us as a concrete reality. Only when we regard it in the abstract does the principle of triple movement seem applicable to *it*. Now how can we account for all this? What can the principle be? Putting together all the facts we have noted, have we not good reason to conclude that it is just the principle of polarity as observed in electric agency? In electric phenomena alone do we seem to experience an opposition which is annulled and at the same time retained. Is not this the principle of movement which Hegel reads into the laws of reason when he regards it in the abstract, and which lands him in contradiction when he attempts to make it concrete? The relation between mind and nature—what is it? Is not nature the negative *induced* by the *positive*, mind, and at the same time *attracted* by it? The powerful logical distinction which moves the world, which is not merely a *distinction* but a *living* power immanent in all physical movement—is it not electrical repulsion? The ultimate unity of the opposition or the "return-into-self"—does it mean anything more than what we understand by electrical attraction? Will not this interpretation give us an insight into the Hegelian philosophy which will clear away for us many of its perplexities, and account for its principal inadequacies?

We have traced briefly the influence of the discovery of galvanism on the scientific thought of Hegel's time. We have seen how the conception of polarity tacitly worked itself into the network of the whole intellectual world and became the central apperceiving thought in nearly all scientific and philosophic speculation. We have seen how the world consequently fell apart into an infinity of polar antinomies. In Hegel's Logic we have seen that his definitions and descriptions of his fundamentel philosophic principle are nothing more than descriptions of the conception of polarity. We have considered this principle as a principle of development, and have found that, though it presents the appearance of a real synthetic movement, it plays no part in the real evolution of thought; that it remains the same throughout the evolution and is all through completely satisfied by the description of polarity. We have examined it as a principle of solution in the problems of philosophy and have found that it does not prove itself a principle of universal reason; that in this respect also it proves itself to be nothing more than the principle of galvanism. Our natural conclusion, then, is this: The age in which Hegel lived compelled him to stand between two great worlds, each full of contradictions. Behind him was the logical world pregnant with the Kantian antinomies of reason: before him lay the physical world charged with the polarities of electricity. The latter being his world of experience, becomes a part of his life, and constitutes his apperceiving thought; and in the life of this world he reads the former. It is thus he unites the two. This is his monism. This is his logic that is at the same time a metaphysic.

What, then, is the value of Hegelism? It is valuable in two respects. First, there is value in the facts which he so extensively and so accurately observes. And from this point of view there is

value in many of his general conclusions, for example, the acceptance, in a very general way, of anthropomorphism as the highest possible world conception, and the interpretation, in the same general way, of thought as a development. In these "generalities," as such, must consist the whole metaphysical value of the system; in Hegel's peculiar rendering of them, in the particular kind of anthropomorphism or development on which he insists, there is none. So, after all, the metaphysical value of the system may be said to be in the aim rather than in the accomplishment; it is ideal rather than real. But, secondly, there is another value which I think is more important and which is specially brought out by this investigation. It is a psychological and pedagogical value. In his endeavor to make the so-called ultimate principles of reason as exhibited in the science of logic conform to his newly conceived principle, Hegel necessarily freed thought from the fixed and apparently ultimate forms in which it had lain bound for centuries. In the successful application of his principle to logic there is involved a deep criticism of the nature of thought, which reveals the fact that the fixed conceptions and so-called ultimate principles of reason are merely finite forms of the "abstract understanding." In this spirit of radical criticism the most final forms of logical and mathematical science are set down as crystallizations of the empirical imagination: retaining, however, their due office as stages in the development of thought. In this criticism of the old forms and in the substitution of his own principle as the final form of thought, Hegel, no one can doubt, has met with wonderful success. Now what is the underlying possibility of this success? Hegel would have us believe that the secret of the whole matter is that he has discovered the one fundamental principle of reason. But our examination of the nature and application of this principle will not warrant this belief. The great truth revealed as a result of Hegel's successful treatment is not, as he professes, the *positive infinity* of thought, but rather its wonderful *plasticity*. The truth of the freedom of thought has been revealed, but not in the sense that the principle of freedom or positive infinity has been grasped. The work done by Hegel may be regarded as a great psychological experiment, through which he brings to light what a wonderfully plastic thing thought is. And in this consists the great educational value of Hegelism. A thorough study of it brings into activity the latent plasticity of the mind, thus lifting it out of its old ruts and prejudices, and giving it in consequence a spirit of independence and freedom.

If our main thesis is true, its value will be not so much its own truth as what it suggests. We are led to question that the fundamental principle of the universe has yet been discovered. Has philosophy yet attained that universal standpoint which it claims as its own, or is it still only one of the great number of things that go to make up the sum of life? The aim and spirit of philosophy, the aim which seeks to know the essential nature of things and gain a world-conception devoid of presupposition, we can safely endorse, but are we sure that the methods used are the best for realizing that aim? How far has the aim been accomplished? As we look over the various systems, do we find them presuppositionless? There seems to be an imperfection in the philosophic method, which leaves it satisfied with bringing to light some dominating presupposition of thought, without inquiring into its nature and history. It leaves it uncriticised and regards it as ultimate, when in truth it is far from being so. Thus what Hegel does is to bring into consciousness the central presupposition or apperceiving

thoughts of his intellectual world: but he does not ask what it is. By psychological criticism, we found it to be the conception of electric agency. In the history of galvanism it has its history. If, then, the imperfection of the philosophic method leaves it satisfied with *revealing* the presupposition, is it not the business of psychology to make good this imperfection by telling *what* it *is*? Psychology will not take the place of philosophy, but it can criticise and correct its methods. The philosopher must be a psychologist. In the history of philosophy there are scores of categories which, one may safely conjecture, have a psychological history. There are "unities" and "higher unities" and "double-faced unities;" "subjectivity" and "objectivity" and "subject-objectivity;" "self" and "not-self" and "return-into-self," etc., etc.; all undoubtedly significant and useful to a certain extent. But who can fully realize what is meant by them? Are they not all presuppositions awaiting psychological criticism? Again, in the history of science, if the discovery of galvanism has furnished the psychological history of one system of philosophy, may we not find the histories of others in the development of gravitation, wave-motion, and other epoch-making discoveries? Is there not a great work suggested here—a work that may be of inestimable value to both psychology and philosophy? What new relation it may reveal between these two departments of knowledge, and what benefits may accrue to both, will be seen only when the work is done. But the foretaste of the results is sufficient to warrant the investigation. Philosophy would at least be made conscious of its prejudices and delusive metaphors, and thus be equipped for a revolutionary advance to a higher standpoint. Psychology may find in the history of philosophy psychological phenomena in the widest bearings and most highly developed stages; as well as obtain the results of naturally-performed experiments which are unattainable in the laboratory.

I have much pleasure in expressing my indebtedness to President Hall for first suggesting that I should investigate the discovery of galvanism, with a view to finding the psychological basis of Hegelism; and also for valuable direction in the investigation.

NATIONAL DESTRUCTION AND CONSTRUCTION IN FRANCE AS SEEN IN MODERN LITERATURE AND IN THE NEO-CHRISTIAN MOVEMENT.

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In order better to understand the intellectual and moral state of the France of to-day, let us cast a glance over the past centuries.

In the XVIIth century the Christian faith was sovereign. A creed universally received, guided and limited intellectual activity. It was the time of the great pulpit orators, Bossuet, Bourdaloue, Fléchier, Massillon; the time of Fénelon, Racine and Pascal, all directly inspired by the Christian church. Scholasticism had not yet been superseded. The philosophers of the coming era, Descartes and Malebranche, took great care not to shock the received religious beliefs.

During the XVIIIth century the spirit of France changed. The materialistic philosophy triumphed with d'Alembert, Diderot, Helvetius, d'Olbach, Condorcet. There are no more Christian orators. The pious Racine has been succeeded on the theatre by Voltaire with his sardonic laughter. It is his spirit which dominates the century. Rousseau preaches his crusade against civilized society. If the leaders of the century be no more Christians, they are still dogmatic. The infallibility of reason and the all-sufficiency of science are the two articles of the new creed. These convictions had their great day during the French Revolution, and culminated in the cult of reason symbolized by a woman seated on the altar of the Christian faith dethroned.

The first half of the XIXth century is in striking contrast with the century just elapsed. Pure intellectual life had decreased during the closing troublous period.

The wars of the Revolution and of the empire, which led the French youth through the whole of Europe, brought in a new spirit and new morals. The multiplicity and the diversity of the points of view acquired during these travels in foreign and little known lands, induced dilettantism. After having lived for a quarter of a century of an intensely active life, breathed the fiery air of the Revolutionary period and drunk the intoxicating waters of Napoleon's successes, France was suddenly reduced to the tepid savor of passive life. Haunting dreams of grandeur, the need of quenching a thirst for strong sensations, the abundance of the physical energy accumulated during the gigantic struggles of the empire, produced a general malaise and a longing which found their expression in the literature of the time known as "Romantisme."

Science was progressing rapidly. The natural sciences had their great first representatives in Lamarck, Cuvier, Geoffroy Saint-Hilaire. Philosophy had Jouffroy and Auguste Comte. The Positivism of the latter and English Determinism tended to quiet the

restlessness. The Christian faith did not regain its lost ground, but it was more kindly treated.

As the century passes, men perceive more and more clearly that reason and science have not kept their promises. No more offerings are brought in open daylight to the Goddess Reason. Doubt and pessimism rise. Schopenhauer contributes his theories of which France was certainly not in need, and Renan adds to it his indifferent tranquillity and his nice taste for mystical sensations, and so we reach the débâcle of 1870.

Literature had changed with the development of the exact sciences and the accompanying agnostic tendencies in philosophy. The fantastic world, in which the over-excited imagination of the Romantiques lived, vanished with them and sedate Naturalism replaced it. Its programme is the reproduction of life as it is, in all its crude reality. Flaubert, the de Goncourt, Zola, etc., lead the movement. Each one depicts to us a parcel of our miserable humanity. They all agree to make of life, of man, of woman in particular, a disheartening picture. No more ideal; it is all flesh, lust and fatality. Very happily this literary school by which France has been infected during the past years is doomed. A reaction against it has set in.

The Frenchman glories in the fact that his fatherland directed the chariot of progress in the past ages. He is gifted with a more delicate, a finer nature; his mind is more pliant, more subtle, than that of his German or English neighbors. He cannot live as easily as they of an exclusive intellectual life, for he is more true to the double unity of human nature and does not so easily sever his emotional from his intellectual life. His inertia is less, and he answers quicker to internal or external stimuli. Consequently, his oscillations are more numerous and more rapid. While the English people have not yet reacted to the practical conclusions of determinism, the French people have already tested it, experienced its insufficiency to satisfy the deepest needs of the heart, and are now struggling to shake off this incomplete philosophy by which they cannot live, in order to put in its place some belief which shall give rest to their tormented souls.

It seems to us that once more France is pointing out the way for a nearer approach to the practical truths of life.

The objectified consciousness of the race, as expressed in the Roman Catholic Christian religion and in Positivism, has been overgrown. The former does not answer to the enlarged mental life; the latter proves insufficient inasmuch as it does not recognize the claims of the religious nature. The French are left without a belief by which to direct their lives; they are adrift. They know it, and their conscious wandering in a world without issue and without meaning, vents itself in pessimism. Sensualism and, rarely, stoicism, in which they seek refuge, are not remedies but only phases of the disease.

What way out of this crisis will nature find? That is the question with which we are concerned in these lectures. We shall first seek, in literature, which is the expression of society, what is the spirit, what the moral and intellectual tendencies prevalent in French life; and subsequently, we shall consider the neo-Christian movement, its significance, and the claim it makes for the solution of the problem.

It is not our purpose to thoroughly perform this double task. Our ambition is limited to giving some idea of the remarkable transformation through which France is passing.

I. Artist Sensualists; the Quest for New Sensations; Nihilism and Pessimism.

We begin with an extreme case of perverted sensualism, a person who, no doubt, does not represent exactly a great number of Parisians, but who, nevertheless, indicates well an existing tendency. Remember that we move in the literary sphere, but do not believe on that account that what we shall say refers to or affects only a small group of persons, for in France nearly every one has a bit of literary culture, and every one is more or less directly—much more directly than in the United States—under the influence of the spirit which dominates the literary productions.

J. K. Huysmans, a novelist, called a great artist by many critics, published in 1892 a novel entitled "*A Rebours*" ("Turned around" or "Upside down"). It relates the life of des Esseintes, a Parisian gentleman; perhaps the author's own life with a little added relief. In the following *résumé* of the book, we shall follow as much as practicable its phraseology. The style, broken, rough, jerky, as the movement of an hysteric, intensely colored with a superabundance of strange words or strange combinations of words expressing some uncommon sensations, corresponds very well to the content of the book.

Des Esseintes was born of parents already affected by neurosis. He rapidly gets sick of the people with whom he is compelled to associate during his studies. He then seeks some palatable society among the men of letters. They, too, soon give him the nausea by their banality. Finally, after having tried divers milieux, "he understands that the world is mainly made up of swaggerers and imbeciles." A single passion, woman, could have held him in spite of that universal disdain, but this passion, too, was used up. After having drunk to the last drop the illicit passions, "he came to practice the exceptional amours, the deviated joys." Then the end came; exhausted, his senses fell into lethargy. Impotency was approaching. Abominably tired of life, he withdrew at some distance from Paris, in a house furnished especially for the enjoyment of his satiated senses. There he sleeps during the day and is up during the night, for "the mind does not really get excited, and does not well crepitate except in the contact with darkness."

Days and days were spent in the choice of the colors of the furniture, of the carpets, of the draperies and tapestry. He wanted colors which would stand out in the factitious light of the lamps. Colors such as would give pleasure to weakened and nervous people whose sensual appetites need highly seasoned food. Orange, that irritating, diseased color, with its fictitious splendor, was finally decided upon for the dominating color; all the others were to blend with it.

Every part of the apartment was conceived to awaken in him vivacious and *bizarre* sensations. His dining-room simulated a steamer's cabin. Instead of windows, he had port-holes behind which an aquarium gave the illusion of the sea.

He did not want his bed-room simply rich and voluptuously comfortable; luxury is too insipid, and plain sensuality too vulgar. He desired to make of his chamber a monk-cell, but without the austere ugliness of these abodes of penitence and prayer. To conciliate these conflicting notions, "he arranged with gay objects a sad thing," or rather while preserving the natural ugly appearance of a cell, he contrived to give to the whole room a sort of elegant and distin-

guished air. Instead of a wash-stand, he made use of a piece of antique church furniture in the interior of which a urinal could find place; on it a prayer book remained in permanence. Only genuine church candles, reserved for the church services, were used in that room. One can easily imagine the *bizarre* sensations that such objects, put to such service in that pseudo-cell, would awaken.

The artificial appeared to Des Esseintes the distinguishing mark of man's genius. Nature has had its time; it has for good and all wearied, "by the disgusting uniformity of its landscapes and of its sky," the patient attention of the refined set. The moment has come when the artificial should be substituted to this "everlasting dotard." Des Esseintes praises himself with being an artist in all things. His literary preferences are for the writers of the Latin decadence. Lucan and Petronius are his favorites. Virgil is a vulgar pedant. However strange it may appear, our hero reads the church fathers, some of them at least. The Apologetic and the Treatise on Patience of Tertullien interest him. He reads with pleasure the Christian eloquence of Bourdaloue and of Bossuet, and also Pascal, whose austere pessimism and painful attrition go to his heart. It is useless to say that Barbey d'Aurévilly is among his friends, this wondrous cynic in whom bigotry is allied to sacrilegious impiety. Listen to his analysis of the charm of this diabolical union: "This state, so curious, does not consist only in wallowing in the excesses of the flesh . . . . . it consists essentially in sacrilegious practices, in a moral rebellion, in a spiritual debauch, in an ideal Christian aberration; it resides also in a joy moderated by the fear of punishment. The strength of Sadisme [from the Marquis de Sade] lies then in the inobservance of the Catholic precepts, nay more, in the following of them inverted, in committing, in order to mock the Christ, the sins which he most expressly cursed:—the pollution of the cult and carnal orgy."

Des Esseintes cultivated and enjoyed every one of his senses. He had in a little closet a set of small liquor barrels; by a clever contrivance, he could combine at will the liquors in his mouth. Every one of them corresponded to the sound of some instrument. The dry curaso, per inst., corresponded to the clarinet, "whose song is sourish and velveted; the krummel, to the hautboy, whose sonorous timbre speaks with a twang; the kirsh-wasser blows furiously the trumpet; the gin and the whiskey break through the palate with their screaking bursts of sound." Through skillful experiments, he had acquired the ability of playing on his tongue "silent melodies, mute funeral marches; to perform in his mouth solos of peppermint, duos of vespetro and of rhum," etc.

To pander to his sense of smell, he had had prepared perfumes with which he filled his apartments. It was now a rain of human essence, smelling of woman; then the scent of a manufactur or of chemical products; at another time, he injected in the room an odor which he called essence of blooming fields.

It goes without saying that physical troubles accompanied this extravagant life. Hallucinations of smell and of hearing, noises of the arteries, a dry regular cough, followed upon each other; later came irrepressible vomiting. Death stared him in the face. The doctor ordered peptone anal injections. Des Esseintes could not help, in spite of the extremity to which he was reduced, congratulating himself on this event which crowned the artificial existence he had arranged for himself. It would be delicious, thought he, if, after health regained, one could continue this simple way of taking food. Instead of spreading the table, one would have simply to set

down upon it the magisterial instrument and, in less than the time necessary to say grace, the repast would be over and the annoying and vulgar drudgery of an ordinary meal would thus be avoided.

At the idea that to avoid death he must of all necessity go back to Paris, and live in the company of his fellowmen, Des Esseintes falls into the blackest despair. Man's society is utterly abhorrent to him. Under this calamity the hunger for faith, which had showed itself repeatedly previously, becomes tyrannical. "Now that he had to re-enter life, he would have liked to be able to compel himself to possess faith, to incrustate it in himself, to screw it down in his soul, in order to shelter it from all those arguments which shake and uproot it. For des Esseintes, faith is belief in the Roman Catholic church; his religious notions go no further. But the more he craved for it, the more did Christ delay its visitation." His upward flights were repeatedly crushed down by one or the other "of the cursed discoveries which have destroyed the religious edifice from top to bottom since two centuries." It occurred to him, per instance, that shameless merchants made the sacred host with potato fecula, "now God refuses to descend in fecula. That was an undeniable fact; in the 2d vol. of his Moral Theology, His Excellency the Cardinal Gousset had treated logically this question, and according to him, potato fecula was in no way a competent matter for the Holy Sacrament. That perspective of being constantly duped, even at the holy table, was not made to deepen weakly grounded beliefs; and, moreover, how can you conceive of an omnipotence which finds its manifestations arrested by a handful of fecula or a taste of alcohol?"

However ridiculus this argumentation, however farcical religious needs thus expressed and thus repulsed may appear, we have here the representation, more or less exact, of the religious state of a large class of intelligent persons in France. Religion to them is the Roman Catholic faith; outside of it there is no religion. And the Roman Catholic religion is a whole, no part of which is non-essential, no part of which can be detached without destruction of the whole. This conception is indeed the sign of a very primitive, or rather, of a very abnormal religious development. But it exists and testifies to the perverse notions which Roman Catholicism and moral corruption have diffused among the people.

In spite of all objections, des Esseintes sees more and more that the reasonings of pessimism are powerless to alleviate his misery and that the impossible faith in a future life alone would give him peace. In a moment of half morbid anguish he exclaims, thinking of the life he is to resume in the midst of society: "Alas! Courage fails me, and my heart heaves. Oh, Saviour, have pity on the Christian who doubts, on the unbeliever who desires to believe, on the convict of life who must embark alone in the night under a starless firmament." These are the last words of "A Rebours."

Let us pass now to one of the precursors of the literary school which inscribes on its banner "Décadents." Towards the middle of the century a writer already known as the translator of Edgar Poe, *Charles Baudelaire*, published a volume of poetry with this significant title, *Fleurs du Mal* (Flowers of Evil). The time was fortunately not yet ripe for such productions. The volume aroused a tempest of indignation, and the author was prosecuted on the score of extravagant immorality.

It is to-day well-nigh impossible to talk French literature without at least mentioning Baudelaire. This is not the place to speak of his poetical talent; we are concerned only with his ideas and the moral

character of his works. Let it suffice to say that there has probably been no French poet who surpassed him in the powerful uses he made of the language. He fashioned a style which made possible the rendering of a series of things, of sensations and of effects unnamable and unknown before him. Every person who feels an artistic vocation believes it his duty and his privilege to admire his talent, whatever he may think of the author's moral nature. Baudelaire is the leader, and in some degree the source of inspiration of a large number of the most gifted artists in letters who came after him. He is the spring, where the literary youth craving for fame go and drink at their entrance in life. On account of that controlling influence, we have thought well to say a few words concerning him.

The whole of Baudelaire is in the *Fleurs du Mal.* The peculiar perfume which they exhale has no name. It is as a bouquet grown on a carrion, or, as some one has said, "an Eden of hell, where Death walks in company with Voluptuousness, her sister." The brilliant Théophile Gauthier said, speaking of the book, "To give to the taste an unknown sensation is surely the greatest happiness (bonheur) that can fall to a writer, especially to a poet." Baudelaire has certainly had that joy. Another literary man exclaims, "You have lighted the artistic sky with a *rayon macabre;* you have created a new shudder." The same Gauthier writes, "In order to depict these corruptions, he has found those morbidly rich tints of more or less advanced rot . . . . . those roses of phthisis, those whites of chlorosis, those bilious yellow, those poisonous metallic greens, stinking copper arsenate, ..."

"L' Invitation au Voyage" is of an enchanting languor; it is as music to the ear, as a voluptuous caress to the flesh. In the land where he proposes taking his lady companion:

"There, nought but order, grace, is found,
And pleasure's calm voluptuous round."[1]

In "La Cloche Fêlée" (the Cracked Bell), he compares himself to a bell tolling in the mist. "My soul is like a bell that's cracked, and when beset with cares, it fain would people with its songs the cold night airs. Not seldom its feeble and weak voice appears the rattle of some wounded one, forgotten and lone, who, beside a lake of blood, corpse-covered lies and dies, stirring not, amid a world of efforts." In his dedications, he does not forget Satan; to him he addresses litanies, beginning thus:

"O thou of angels all the fairest and most wise,
God by Fortune betrayed, bereft of eulogies,
O Satan, take compassion on my long distress!"

and this hideous supplication is repeated after every two verses:

"Adopted father thou, of those whom God has driven
In anger dark and fierce forth from the earthly heaven,
O Satan, take compassion on my long distress!"

He eulogizes the sterile woman, addresses his salutations to a certain "Queen of Sin," and delights in exciting descriptions of carnal feminine charms.

"Dear indolent, how fair a sight
Thy grace of body seems!
How like the stars' inconstant light,
Thy skin's soft gleams!"

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[1] These translations from the Fleurs du Mal we owe to Dr. Chamberlain.

In his morbid malaise, lashed by unquenched desires, he exclaims: "No soul can fit a heart so deep and dark as mine but thine, Lady Macbeth, potent in crime and wrong." The following is an attempt at rendering perhaps the most strongly written sonnet in the French language. A sensitive nature could hardly read it in the original without remaining for long hours under its sinister impression.

SPLEEN.

"When the low-hanging sky like a dark cover weighs heavy on the groaning soul, a prey to griefs and cares, and, embracing the wide horizon's round, pours on us a day more dark, more sad than night,"

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"When the earth is changed into a humid prison-cell, where hope flits to and fro like a poor bat, beating in aimless flight the walls with timid wing, striking its little head against the mouldering roof,"

* * * * * * *

"When huge trains of rain-drops in their fall mimic the bars of some vast dungeon, and a mute folk of horrid spiders cast and spin their webs deep in our brains,"

* * * * * * *

"Suddenly and furiously bells clash forth and fill the skies with frightful howls, resembling wandering, homeless spirits who in stubborn groans vent their long woes,"

* * * * * * *

"And through my brain moves the long procession of hearses, slowly, without the sound of music or of drum; hope, vanquished, weeps, and anguish, cruel and despotic, hoists her black flag over my prone head."

A few more words on the ideas of our poet. He was, it is useless to say after these quotations, a fatalist and a pessimist to the core of his heart. Progress, the great modern idea, was for him "an ecstasy of fly-catchers." "He held in profound horror philanthropists, utilitarians, humanitarians, and all those who pretend to change anything to the invariable nature and to the fatal organization of society." He loved the artificial, the after-touches made by art to nature. To a simple young girl, he preferred a ripe woman adorned with all the art of a learned coquetry.

His finely moulded nose, rather soft, with delicate palpitating nostrils, indicated well the subtlety of his sense of smell; "My soul hovers on perfumes as the soul of other men do on music," he used to say. One of his sonnets expresses beautifully his sensuality of smell:

CORRESPONDENCE.

"There are perfumes as fresh as little children's flesh,
Sweet as hautbois, green as the meadows,
And others, corrupt, rich and triumphant,
Having the expansion of infinite things
Like amber, musk, benzoin and incense,
That sing the transports of spirit and sense."

Charles Baudelaire died from paralysis. To-day a loud cry is raised by some persons asking that a statue be erected on a public place of Paris to this man whom justice prosecuted a few years ago.

School of the Décadents.

But let us hasten further to the *School of the Décadents.* During the past ten or fifteen years a little group of blustering young men have caused a prolonged stir in France, and have drawn on them the attention of the public and of many of the literati by the eccentricity of their productions and of their theories. This movement is, it seems to me, highly interesting as marking the complete dissolution of all beliefs, of all restraints, of all rules, even of those rules which seem the most deeply rooted and the most necessary: French lassitude, French, or rather Parisian skepticism, no more a theoretical skepticism, but an assimilated skepticism, truly ruling and governing man, have no better example than this literary manifestation.

Some poetical talent is found among them combined with pretensions that call to mind the illusions of general paresis. *Baju, e. g.*, in a pamphlet glorifying the Décadents, boldly declares that Naturalism has delighted those who are incapable of seeing and of feeling in any other way than by their senses. Naturalism is without ideas, it wallows in matter. To the Décadents was reserved the honorable task of crushing Naturalism and of creating a better taste, no more in contradiction with modern progress. He gives to the school a reformatory mission. "It attempts," says he, "to elevate the moral and intellectual level of the masses assailed by a deep disgust and an incurable spleen." This is no buffoonery; the writer is in dead earnest—or at least in as great earnest as he can be,—but he evidently is in the deepest confusion as to what morality is. He gives a satisfactory proof of this a little further, where he eulogizes Barbey d'Aurévilly, who so cleverly unites the sacrilegious to the holy. "Barbey d'Aurévilly is, if we believe Baju, the most colossal thinker of all ages. He is truly *the* writer of the century. Victor Hugo, who is held to be a giant, is nevertheless a dwarf by his side."—Enough of this.

What are the theories or the principles of the Décadents? They claim to be a school; they must hold some common doctrine, and so they do. Let me say, first, that the various men and the considerable literature generally named by this appellation, make up a heterogeneous mass without unity beyond a profound contempt for all received rules in the art of writing and a licentious refinement of sensations.

I transcribe two authorized passages containing their programme. One from *G. Kahn*: "We want to objectivate the subjective, viz., to project the idea instead of subjectivating the objective, which means nature seen through a temperament." The other from a poet: "We want to reach into the essence of nature, the manifestations of which glitter on the surface of things." Baju has a very clear idea of what the Décadent literature should be. "It takes up only what directly interests life," says he. "No description; we suppose all known. Simply a rapid synthesis giving the impressions of the objects. Do not depict, but make the reader to feel." But the most complete exposition of their method is found in the *Traité du Verbe of René Ghil.* It is a sort of rhetoric of Décadentisme. Unfortunately for the propagation of its contents, the pamphlet is generally unintelligible. It is written in a new language; the words are still, in parts at least, French words; but the sentences are constructed according to a syntax of their invention. The initiated only can perceive the new light concealed in that obscurity. Nevertheless I shall attempt, in due humility and diffidence, to give a *résumé* of the teaching of the *Traité du Verbe.*

First principle: use the *Symbol*, viz., do not stick close to reality, but extract from it its essence, that which moves us. Instead of making a long description of a beautiful landscape write simply a few words which will convey the total impression. The words need not be connected in any way, provided they give the desired sensation. Hear how well the poet succeeds in conveying the *impression*. This is the translation of the first strophe of one of his poems, called "The Blood at the Temples." I dare say that my translation is no more incoherent and no more obscure than the original.

"Alas! in the rugged dance, where trunks go naked in the manufacture thundering hard, loved rumors, for it we go, then, the rigid steam having you at both fists, Ô masses to the long flight, no more to waltz the waltz at the high supreme whirling?" A powerful imagination, not fettered by too much hard common sense, might find an interpretation to this poetry.

Second and last principle: use the *Verbal Instrumentation*. The vowels, as the musical instruments, have each their distinctive character. By a clever combination of the vowels and of the consonants, symphonies capable of awakening in us the most varied sensations can be produced. Our author has definitely determined, with an admirable precision, the musical correspondent of each letter. Moreover the vowels have also a color meaning. We reach thus the following table representing what René Ghil calls the Verbal Instrumentation. It is at bottom an extravagant system of imitative harmony or rather, of imaginary harmony.

F, l and s, correspond to the long, primitive flutes. L, r, s, z, correspond to the horn, bassoon and hautboy, etc. . . . .

Ô, o, io, oi, give the reds. Oû, ou, iou, oui, go from the black to the russet, etc. . . . .

The a, o and iu, are to be used to express magnitude, fullness and amplitude. E and i, for the tiny, the sharp, the sorrowful and mourning. O, r, s and x, for the great passion, for impetuosity, roughness, etc. . . . .

Everything is clear and simple; and now, when a poet seized by inspiration is prompted to sing, he needs only open Ghil's Table and following it, combine vowels and consonants according to their indicated natural meaning, to express, with all their nuances and subtlety, whatever emotion may oppress his soul.

I cannot refrain from mentioning an attempt made at a Paris theatre to make use of these fanciful discoveries. M. Rounardo and Mme. Famen de Labrély have adapted the "Songs of Songs" of King Solomon to the stage. An actor dressed in yellow comes to the foot-lights and delivers his part, in which, through a happy choice of words, the same vowel, i per instance, recurs constantly. That vowel is supposed to suggest the color yellow (according to Rimbaut's color-alphabet, I think). Other declaimers, attired in garbs of divers colors, deliver speeches in which corresponding vowels dominate. The color-tone of the stage decoration changes to match with the artists and with the part of the piece being recited. Furthermore, to complete the harmony, a symphony in *re* is heard, and perfume of white violet is crushed to powder near the prompter's box while the speech in i goes on. The music and the perfume also change with the part recited. This was a very candid and logical attempt. It is not necessary to say that it did not meet with the success expected.

Paul Verlaine is claimed to be the greatest living representative of the Décadents' School. Some young literary men have almost

deified him in their frantic admiration. One of them called him the greatest thinker of all times. Physically Verlaine is a somewhat extraordinary being, with a Socratic profile, a forehead immoderately broad, and a skull covered with bumps. His life is full of obscure events: one day, for instance, he disappeared and for ten years remained hidden. Some said that he spent them in prison, others, in a hospital. Recently he published, after a long silence, a little volume of poems, *Wisdom;* but he still lives hidden, nobody knows where, possibly in the rear of some barkeeper's shop.

Les Poèmes Saturniens, *Les Fêtes Galantes*, *Jadis et Naguère*, *Romances sans Paroles* and *Sagesse*, are among his important works. Verlaine is not at all lettered; he uses the words after his own ways without caring for rules. He is sensitive as a child, and contrary to the assertion of his admirers, is a very poor thinker. His poetry expresses only sensations and feelings. It seems as if he was writing for himself alone, and in fact he is sometimes unintelligible. It is astonishing that such a reprobate can express feelings so sweet, so pure, that they seem to proceed from a virgin soul. Can this be artifice or refinement? Neither the one nor the other. This mild Décadent seems to have remained a child through life, or perhaps, after having tasted of all the pleasures of life, he returned to the primitive condition of his soul. *Sagesse* contains the effusions of a repentant sinner who returns to religion. Here the childish turn of mind of the poet shows itself plainly. One cannot conceive of a more ingenuous faith, of a humbler submission. He accepts without a question all mysteries and all dogmas of the Roman Catholic church. I wish I could translate here some of his inspired verses; but since time does not permit, we conclude with the opinion of Anatole France, expressed after the reading of *Sagesse*.

"Thou hast erred, but thou hast confessed thy sins. Thou wast an unfortunate man, but hast never lied. We are Pharisees; thou art the best and the happiest . . . . . Paul Verlaine has written the most Christian verses which have appeared in France." But human nature is weak, and I fear that since this very Christian sentiment was uttered, the poet has given occasion to the Critic to repeat the proverb: "The sow that was washed returned to her wallowing in the mire."

Among the prominent Décadents we will further mention: *Stephane Mallarmé*, professor of English, translator of Edgar Poe. He is, after Verlaine, the most talented of the Symbolists. Unhappily he is a great deal less intelligible than the latter.

Arthur Rimbaud, whose sonnet of the *Voyels* is famous.

Stuart Merrill, one of the few easily understood Symbolists. *Les Gammes* contains charming passages.

LITERARY CRITICS.

We are next to consider a group of writers less extravagant, but more important than the preceding; they may be taken as the representatives of the mass of the Parisian people. They are the famous literary critics, *Lemaitre*, *France* and *Sarcey*. Their influence is very great. By their daily articles they make or destroy reputations; they taboo or set in vogue a new book; they mould the taste of the people. They are, par excellence, men of the moment; they speak of the events of the day without an apparent thought for the morrow. They are the people's favorites because devoted to the people's pleasure.

But preliminarily let us cast a glance on the masters and educators of these men, to see from what lineage they have issued.

Among the philosophers we have *Renan* and *Taine*. "We place our title of nobility in this obstinate affirmation (the ackowledgment of duty); we do well, we must hold to it, even against evidence. But there is almost as much chance for the truth of the contrary." You have recognized the bewitching skeptic Renan. But you know him and so do you Taine, who said somewhere, "The best fruit of science is a stolid resignation, which pacifying and preparing the soul, reduces suffering to a bodily pain."

Among the pure litterateurs of whom our modern novelists and critics received their lessons and often their inspirations, we shall mention *Stendhal*, *Flaubert* and *Balzac*. These men are, all three, strong supporters of the doctrine of art for art's sake. Their criterion of the beautiful is their own sensations, their own taste; the possibility of its corruption does not occur to them; they do not know what corruption means. "As the beautiful and the useful have no point of contact, an artist must refrain from expressing his opinions on the things of this world," says *Flaubert*. His pessimism is as deep as the sky. "It is strange," says he, "with how little faith in happiness I was born. I had, when yet quite young, a complete presentiment of life. It was as a nauseating odor arising through the vent-hole of a kitchen. One need not have eaten of it to know that it causes vomiting." It is the same odor that escapes from Flaubert's great novels, and specially from *Madame Bovary*, the creator of the realistic novels. Flaubert died in 1880.

Stendhal is a little older (died 1842). A disciple of Coudillac and Helvetius, he is, as themselves, a sensualist and an ideologue. On the chapter of religion he is ferocious. "The only thing which excuses God," says he rabidly, "is that He does not exist." He is a modern by his sensibilty, his analytical mind and his pessimism. He lacks only moderation in his skepticism to be completely of our day. The mellowing influence of a Renan had not yet softened that fierce negative dogmatism. His novels have had and have still a powerful influence. The most important of them are *Le Rouge et le Noir*, *La Chartreuse de Parme*, *Le Traité sur l'Amour*.

In *Balzac*, the great author of the *Comédie Humaine*, nearly the same views, the same general dispositions are found.

Jules Lemaitre has been known for only about ten years. A pupil of the École Normale, he began his career as professor of rhetoric in provincial cities. In 1884 he abandoned pedagogy and went to Paris to make his fortune with his pen. He became a contributor to *La Revue Bleue* and to the *Figaro*, and a little later was appointed dramatic critic of the *Journal des Débats*. His chief literary studies have been published in five series under the title, *Les Contemporains*, and his dramatic criticisms in a series called *Impressions de Théatre*.

M. Lemaitre is a kind hearted gentleman, often affectedly flippant, sometimes smacking of cynicism; his style is always easy and generally sprightly, picturesque and seasoned with delicate wit. Under his apparent unconcern and good humor, melancholy is discerned. His criticism is purely subjective. He gives his impressions with ingenuousness, regardless of their reflections on himself. As his intellect and his senses are remarkable for their refinement, his impressions are as subtle as they are complex and numerous. He has no dogmatic prejudices in art, nor in anything else; for if, to him, the measure of all things is himself, he knows well that men differ, and is ready to concede that anyone is about as near the truth as himself—if there be any truth. As M. Lemaitre never does violence to himself in order to be consistent, and as his supple intelligence sees things from multifarious points of view, it often happens, ac-

cording to his moods and the changes of weather, that he flatly contradicts himself. But what does it matter? According to his subjective mood, is he not just as right in one case as in the other? Occasionally he wittily asks the reader to forget what he has just said, and proceeds to say the contrary.

The faith of our critic is very hard to define. He would willingly repeat these words of his master, " . . . . . God, providence, immortality, as many good old words, a little heavy perhaps, to which philosophy will give a more and more refined interpretation."[1]

Somewhere half playful, half sadly serious, he makes up a creed. I paraphrase it: "I believe that humanity progresses towards an ideal where justice shall be more perfect, suffering less intense and truth better known. I believe that all men are conjointly responsible, and that we love each other as naturally as we love ourselves. I believe that our advantage and our pleasure are found in loving others, in working for those we love, and, beyond them, for the whole community, amen."[2]

The following betrays in an interesting way the moral state of literary France. Reviewing a book, Lemaitre says: "It is a beautiful book, and (let not the author take this for a lesser compliment), it is a good book." Our good Lemaitre knows that it is so little the custom to give to the good the priority on the beautiful that he feels it necessary to say that such is his opinion.

Speaking of the remarkable book of Edouard Rod, *Le Sens de la Vie* (The Meaning of Life), and referring to a vigorous page on the noxious effects of dilettanteism, our critic thinks "that it was worth the while to describe that evil, if it were only to make us ashamed of it and to incite in us the desire of shaking it away and of passing from the books to active life," but a little further he adds, "And yet, everything being considered, it is to me extremely difficult to be persuaded that dilettanteism is in itself injurious, and I almost feel disposed to take its defense."

He quotes frequently from the *Imitation of Jesus Christ*. He is acquainted with St. Augustine and makes use of his pious sayings. Often for half a page or more, it seems that you are reading a book of devotion. "To love God," says he, "is to love the human soul, aggrandized with the joy of enlarging it unceasingly, and to measure our own value by that growth." Sometimes about a new drama he shows himself austere and speaks humbly but strongly in the defense of outraged virtue. In all this he is perfectly sincere. It is one part of his double nature which speaks in such occasion; for, like every one of us, he is double and he differs from the common only in a greater instability. He allows his other self to express itself just as freely: "Everything well considered, there are three lives worth living: the life of the man who dominates over the other men, through holiness or through political and military genius (Francis of Assisi and Napoleon); the life of the great poet who gives representations of things more beautiful than the things themselves and just as interesting (Shakespeare and Balzac); and the life of the man who conquers and enslaves all the women he meets on his way (Richelieu and Don Juan). This last destiny is not the least glorious, nor the least to be envied." The reader does not fail to see that it is the destiny which M. Lemaitre would choose for himself, if the choice were in his power—for a part of his sojourn on earth at least, for our delicate critic would not willingly make himself inapt to taste the sweetness of Christianity. Do you ask why? First, because "the religious curiosity, is in our

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[1] Ernest Renan. [2] Les Contemporains (Rod), 5e Série.

century, one of the most distinguished and one of the best of our sentiments," and secondly, because the gospels have "I do not know what deep charm, mystic and vaguely sensuous." "The modern soul consults all the gods, not to believe in them, . . . but to understand and venerate the dreams which the engima of life has inspired in our ancestors and the illusions which have alleviated their sufferings." This sounds like Renan.

The best and perhaps the most constant part of M. Lemaitre is his compassion, his pity. I remember having read somewhere a statement of his meaning that he would rather die than willingly cause pain to anyone.

In M. Jules Lemaitre dilettanteism has achieved its wretched work. He has no more character, he is hardly a personality, he is but an intelligence, so subtle and so fluid that it seems ready to fall into thousands of disconnected particles. Of will, habit, inertia, he has little or none.

Anatole France, in addition to literary reviews and critics, collected (the best of them) in five volumes under the title *La Vie Litteraire*, has published a number of valuable novels; *Le Crime de Sylvestre Bonnard*, *Le Livre de mon Ami*, *Balthazar*, are the most important.

The following portrait from his pen shows his moral attitude: "Jules Lemaitre is a very wise and very subtle person, whose *happy perversity* consists in doubting incessantly. That is the state to which thought has reduced him. . . . . Thought is an awful thing . . . . . M. Lemaitre has no doctrine, but he has a moral philosophy. This philosophy is bitter and sweet, indulgent and cruel and most of all, kind . . . . . sometimes ascetic and sometimes sensual."

With these two writers, Lemaitre and France, must be associated their colleague, *Francisque Sarcey*, known only by his criticisms. His authority may be judged from the fact that he receives 80,000 francs a year for his weekly chronicle in *Le Temps*.

The first of this trio can be taken as representing the others, so that we shall pass further after the following quotation taken from A. France: "His book (*Mensonges*, a novel of Paul Bourget), in which the inimitable voice of truth is heard, induces despair. Its taste is more bitter than death. It leaves ashes in the mouth. It is why I have gone to the spring of life; it is why I have opened the *Imitation of Jesus Christ* and read the salutary words, "*But we do not want to be saved; we fear, on the contrary, to be deprived from the voluptuous pleasure of going to perdition. The best among us are as Rachel, who did not want to be comforted.*" The attitude of the group of men we are studying could not be better expressed.

Chroniclers.

The Parisian newspapers are much less voluminous than those of the large cities of the United States. In them the most interesting events of the past day, social and political, the public rumors, gossips, etc., are brought together in a tasteful and sprightly chat called the *Chronicle.* The Chronicle is the most read because, to the large mass of the people, the most interesting part of the paper. The chroniclers, for the best papers, are men of talent, clever to please and enjoying a great popularity and a corresponding influence. This forces them on our attention. We shall say a few words concerning four of them, *Albert Wolff*, *Emile Blavet*, *Henri Fouquier* and *Rochefort*.

MM. Wolff and *Blavet* write for the *Figaro*. The first "knows, for the joy and the edification of the people, how to appear in the

same time flippant and serious, boulevardier and moralist, the gentleman who understands all, but who, nevertheless, respects that which must be respected, the gentleman without prejudices of any kind, but, nevertheless, having principles."[1]

M. Blavet knows also exactly what the reader wants of him. Lemaitre says that "he has the gift of catching with agility the fugitive traits of the daily comedy, to amuse himself with it and to amuse others. Not a shadow of pretension, a very philosophical good will; *at bottom an absolute indifference.* This one is a Parisian."

Henri Fouquier writes for the *Gil Blas*. He is more original than the two preceding writers. He is the most distinguished of the Parisian chroniclers. "The mind, the most easy, the most alert, the most skillful, the most ready in all things . . . . He reproduces on the run the most recent way of understanding and of seeing which men have found, as if he knew it from all eternity."[1] His fort is woman. That should be the strong point of every Parisian chronicler, for, as one of them said, "Woman is that which fills the greatest room in man's life." You will be able to lay hold of his moralsin the following quotation on love: "Here is the simple fact to which I want to arrive: there is no social morals, there is only a worldly free-masonry, absurd, with cruel and sanguinary rites against which our heart and our reason protest. To seek the law of the world is truly a folly; there is nothing left to do but to submit to it. That free-masonry holds that a young girl who gives herself for a bouquet of roses is lost, while a married woman who gives herself in a caprice or for a bracelet is not on that account dishonored, provided she plays the hypocrite." These, and the like, are the ideas which M. Fouquier develops for the readers of both sexes and of all ages of the great daily *Gil Blas*.

But little variation is perceived as we pass from one to the other of the chroniclers. It is about the same type. It is interesting to notice that the talents which distinguish them are those more naturally belonging to woman: a marvelous gift of receptivity, a great elegance of language, a fine sensibility and a powerful intuition.

As to *Henri Rochefort*, the publisher of *La Lanterne* (a daily paper), he is a unique being. Lemaitre finds him "a most interesting and, at the same time, most irritating moral case on account of the impossibility of seeing clearly to the bottom of it . . . . . For the past twenty years his hisses have been heard without interruption on the public place. The empire fell under the noise of his rattle, and since then it has never ceased grinding for a single day. The spirit of Rochefort is uninterrupted, methodical and universal irony. One feels very clearly that the secret source of this raillery is not, as is the case of other great scoffers, the love of the truth, of the just or of the beautiful. His raillery attacks every subject; be it a ridiculous thing or an infamous one, the same methodical sneer disposes of it."

Rochefort is a factor in the political world. He is a deputy to the Legislative Chamber. There he defends what he calls the cause of the people, not that he loves them—that does not appear—but because under that mask of defender of the oppressed he finds the means of best satisfying his hatred and his need of destroying. He was the ally of General Boulanger.

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[1] Les Contemporains, Jules Lemaitre.

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The bloody period of the French Revolution known by the name "Terror" had such men. A society which not only produces such monsters but tolerates them, not only tolerates them, but honors them with a seat in its Legislative body, a society which supports and applauds to the perverse turpitude of a political newspaper inspired by such madmen, may not be far from anarchy.

The novelist, *Maurice Barrès*, poses as the representative of his generation, and many take him seriously. He is still quite young, but as Corneille said:

> Chez les âmes bien nées,
> La valeur n'attend pas le nombre des années.

His numerous admirers compare him to Descartes and to Spinoza. Indeed he reads Spinoza.

The only reality which he recognizes is the *Ego* and its sensations. There is nothing real under the words truth, justice. The *Moi* is all; there is nothing beyond. Maurice Barrès has then a very exalted idea of the *Ego*; it represents the conscience of the race; it is a link in an immortal chain. The *Culture du Moi* is the business to which we should all give ourselves; everything, sensations and emotions, are made to help the realization of our being. Laws are iniquitous, for the only reason of life is the free development of the *Moi* by sensations. Laws are a slavery that dead generations inflict on the present generations; a tyranny of the senseless dead on the living who feel and suffer. To regenerate society, it would be sufficient to put all the *Egos* in liberty by abolition of all the laws.

With ideas so profound and so wise, M. Barrès has not had to wait long the honor of representing a part of his country in the Chamber of Deputies.

He developed the ideas here summarily set forth in four books: *Sous l'Oeil des Barbares*, *Un Homme libre*, *L'Ennemi des Lois*, *Le Jardin de Bérénice.*

II. The Tormented.

We arrive, in our survey of modern French literature, to a goodly number of talented men, brought up under the influence of the Flaubert, the Baudelaire, the Renan, the Taine, men who more or less earnestly wrestle against the sensualism and the dilettanteism they have inherited and cultivated in their youth, and who aspire to some faith to guide them to the ideal. In their best moments they throw out the cry of deep distress which a journalist recently uttered: "We have no chapel where we can kneel down, no more faith to sustain us, no more God to whom we can address our prayer. Our hearts are empty, our souls are without an ideal and without hope . . . . . You, who have the good fortune of believing in a Sovereign-ruler, entreat him to reveal himself to us, for we long to suffer and to die for a faith."[1]

We have seen these same desires, less warmly felt indeed, in many of the writers we have mentioned, in Huysman, who points out faith and religion as the only remedy to the woes of his hero, in Lemaitre, and in France. We know that in all times some restless people have spent their life seeking in anguish some peace-giving panacea. But the *Mal du siècle* to-day bears a particular stamp. It is deeper and larger, it is no more vague, immatured and unexplained as in the time of Musset and of Lamartine. It is no

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[1] From *Le Christianisme au XIX. siècle.*

more a mere soaring up towards a mystical ideal; the evil is, it seems, fathomed; its remedy is known.

Everywhere, in the daily papers, in the reviews, in the novels, in poetry, even in science and philosophy, traces of a new spirit are found. About two months ago Madame Adam, directress of the *Nouvelle Revue*, invited M. Raoul Pictet, the well-known physicist from Geneva, now professor at the Berlin University, to deliver the inaugural address at the opening of a new lecture hall in Paris. Before a chosen audience of men of letters and of savants, the scientist related his evolution from the materialistic theory of the universe to the spiritualistic conception. The conclusion of the discourse is well worth repeating, coming from such a man: "After having admitted first the notion of ponderable matter, then that of ether, later the notion of actual movement and then that of potential movement, contemporary science is compelled to recognize still another force, a soul-power, in order to satisfactorily understand the observed and observable facts. Experimental physics demonstrates that morality is possible, that duty and free will can be affirmed and, consequently, that men can escape from the mechanical determinism without upsetting the order of the universe."

The French know no more how to poke fun at religion; on the contrary, they wish for it. Even clericalism comes to be looked upon graciously; the famous exclamation of Gambetta, "Clericalism, that is the enemy," is no more heard. A few months ago the minister of public instruction, Bourgeois, against all precedent, appointed an abbey as director of an important Lycée at Nancy, if we mistake not. The press comments were on the whole complimentary. In the schools the questions of religious faith are not discarded *a priori*. The students are ready to listen. The possibility of an intervention quasi-miraculous of the divine is more frequently admitted. The psychological studies have no doubt been a factor in this evolution and especially the mysterious revelation of hypnotism, telepathy and those of spiritualism. Many expect psychology to throw a bridge between positivism and transcendentalism.

Idealistic novels become more numerous. Lately the *Revue des Deux-Mondes* (1890) published a novel entitled "*Ni Dieu ni Maître*" (No God nor Master), by *George Duruy*, professor at the university, whose theme is the conversion of a physician free thinker. Again here the *Imitation of Jesus Christ* is quoted at length. *Noël Blàche* presented to the readers of the *Nouvelle Revue* (December, 1892) a man of the world, a Parisian boulevardier, and led him to the brim of Christian conversion. In the last chapter the writer places these words in the mouth of his hero: "I feel it now, there is in life something else than race horses, opera women, clubs and the like." He nevertheless continues the same life, for he is unable to shake off his habits. He excuses himself by repeating despondently the old proverb: "Qui a bu boira" (He who has drunk, shall drink again).

Two young poets have recently published songs of hope. One of them, *Albert Jounet*, in two volumes of verses, *L'Etoile Sainte* and *Les Lys Noirs*, gives himself up to religious inspiration. The other, *Emile Trolliet*, issued a volume of poems with a prelude dedicated to M. de Voguë, and a piece entitled *Relèvement*, addressed to M. Paul Desjardins.

A group of young men striving for a literary career has lately founded *la Revue Libre*, a small publication of considerable merit. In it can be found striking examples of the transitory moral

stage, characterized by the new-born mysticism in which sensuous love and spiritual love meet and pass into each other.

Pierre Lasserre, a young author, attempts in *la Crise Chrétienne* to analyze and reduce to their just value the new aspirations with which so many of his countrymen are agitated.

But most significant, perhaps, among the numerous signs of a new orientation is the recent choice made by the General Association of the Paris Students for their president. The election carried to that influential position M. Henri Bérenger, a talented young man in sympathy with the neo-Christian movement. In a speech to his fellow-students we find this sentence, "Let us seek to be imbued with the spirit of Christ." A few months ago he gave to the public a novel, *l'Effort*. It is the voice of one who has suffered, warning his brothers and his sisters. The evil of the present resides, in the author's opinion, in the abuse of thought, in the spirit of analysis. He designates it by the term *intellectualisme*, "that perversion of the mind which reduces us to seeking in life only the spectacles of life, and in sentiments only the ideas of sentiments." Intellectualism destroys intuition, that deep primitive impulse of the soul which is the natural spring of action, and in so doing brings about the dryness of soul and the moral inertia of which France is dying. Even love, the deepest and most essential sentiment, the essence of the soul, is extinguished.

The book is written in the form of a novel. A Georges Lauzerte, the personification of intellectualism, is led to a suicide. Contrasted with him is his friend, Jean Darnay, the man who finds in the intuition of his conscience the source of a saving activity. Duty, for M. Bérenger, is one with love, for we know duty only through impulses. So that duty should not be separated from love.

> "I give nothing as duties:
> What others give as duties, I give as living impulses;
> Shall I give the heart's action as duty?"[1]

The persons familiar with the spirit of the Paris students of a few years ago, will see in the election of M. H. Bérenger as president of the Students' Association the proof of a wondrous change.

Mark, if you please, that the transformation we have pointed out is not due to the intervention of exterior influences; it is not the fruit of the admonitory appeals of those who have remained untainted by the evils of the period, nor is it due to the teachings of the church. The patient himself has found in his condition the reason of a new course of life. We have here a beautiful case of the normal workings of nature: a society having wandered away from true human life in the process of readjusting itself to the laws of life under the incentive of the moral disturbance consequent upon an anti-natural existence.

If, from the preceding general indications, we pass to the group of men we have more especially designated by the term tormented, we find some poets, *Sully Prudhomme* and *Maurice Bouchor;* at least one dramatist, *Alexandre Dumas*, *fils;* some novelists, *Paul Bourget* and *Edouard Rod*. Other names might be added, but these men illustrate sufficiently well the various aspects of the *états d'âmes* to which we desire to draw your attention. In these five men, however they may differ in their self-consciousness, or in their manner of manifesting their moral disquiet, at bottom the same conflict, arising from the same aspirations by the same grievous tendencies, is distinctly perceived.

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[1] From Walt Whitman, as quoted by M. H. Bérenger.

Sully Prudhomme of the French Academy, author of *les Epreuves*, *les Solitudes*, *les Destins*, *la Justice*, *le Bonheur*, etc., is a poet-philosopher. He began long ago with philosophical poems, full of enthusiasm and of confidence, in which he preaches action and censures egoistical despair. Since then his voice has grown more harmonious, but also more tormented; he speaks long-felt miseries. The aspirations toward the infinite, the smallness of man before the starry vault of heaven, the anguish of doubt, are the themes to which he constantly recurs. Often he rises on the wings of hope, never on those of faith. In the beautiful stanzas, entiled "le Voeu" (the Vow), he exclaims in a burst of passionate compassion for unhappy mortals:

> "Du plus avengle instinct je me veux rendre maître,
> Hélas! non par vertu, mais par compassion.
> Dans l'invisible essaim des condamnés à naître,
> Je fais grâce à celui dont je sens l'aiguillon."

In opposition to the theory of art, for art's sake, stands the playwright, *Alexandre Dumas*, *fils*, also member of the Academy. In his estimate, "All literature which does not have in view the perfectibility, the moralization, the ideal—in a word, the useful, is an unwholesome literature." Every one of his tragedies or comedies is a moral thesis, whose theme is nearly always woman, her moral nature, the rôle she plays and the rôle she should play in society, adultery, that sentimental and elegant prostitution received as a poetical weakness. Society receives the announcement of a new piece of Dumas about as the church-going public of New York receives the information that such a well-known preacher is going to deliver a sermon on the social evil.

Let us find from his writings the ideas on love and on woman of this would-be reformer. The relation of the sexes is for him *the* social question; all others are subordinate. The world revolves around sexual love. Lewdness, the seeking after voluptuousness is the great, the only great danger of the present. He often agrees with Schopenhauer. I doubt not that the German philosopher is responsible for a part of the utterly disrespectful notions of the academicien concerning woman. "True love is a very rare thing, rare as true genius, as true virtue, as everything that is true. Many are called to it, but few are chosen." Marrying is only making the best of the worse. Marriage, says he in a sally, is a means of transportation, the omnibus which conveys us to the end of our life's journey. The passengers are tossed about, shaken up, vexed and annoyed in many ways, but better suffer the less in the coach than experience the fatigue of walking and running the risk of losing oneself in a roadless country. In the preface to *l'Ami des Femmes*, he draws a very dark portrait of woman. "Woman is a circumscribed being, passive, a disposable instrument in perpetual expectation. She is the only incomplete work which God has allowed man to take up and to finish. She is a riffraff creature. . . . . Woman will no more be a wife, a companion, a friend, a slave, a victim of modern society; she is first of all an adversary. . . . . There is no family in the civilized world which, at this hour, has not to defend itself against this insurgent, woman." On the question of emancipation, Dumas becomes highly entertaining: "Independently of man, woman does not act, she flutters. . . . The emancipation of woman by woman is one of the most exhilarating jollities which ever came to life. It is pure nitrous protoxyde; uncorking suddenly, the flask would set God laughing for eternity." I quote these buffooneries to point out the tone of

this stage-moralist. One of his elements of success will now appear more clearly.

M. Alex. Dumas tells us somewhere how he became a moralist: "One could not have, unless he be crazy, the pretension of achieving all alone a general reform; it is probable that this reform must advance gradually. So that a person willing the good will chose any one of the numerous points at which the symptoms of the quasi universal imbecility manifest itself, and, directing his attention to it, will make it his point of attack." Our playwright chose woman, and took upon himself the mission of reforming society through her. The theatre became his battle-ground, and since that early day he has not ceased writing and writing with a *brio*, an abundance of animal spirit and of wit, truly bewildering.

You have not failed to notice in the last quotation the term "symptoms of quasi universal imbecility." Here, again, the point of view of our moralist is apparent. Do not these words indicate, what is felt through the whole of his work, that his intelligence much more than his conscience, or his heart, is galled by the folly of man? No moral reformer ever subsisted on intellectual sentiments. Nevertheless some have awarded him the title of spiritual director of this century. This shows only how much France needs a spiritual director.

It cannot be doubted that Alex. Dumas is in earnest, but it is not the earnestness of a person conscious of moral evil. There is in him too much blustering, too much fondness for scenic effect, and too little hatred of sin. In truth, he is not enough of a saint to reform anything or any one. The public will enjoy his brilliancy and his daring and often immodest wit, but will not go further. Alex. Dumas is a sort of modern literary Don Quixote.

In the poet *Maurice Bouchor*, we witness the moral evolution through which so many young Frenchmen pass.

At the early age of 18 (in 1874), Maurice Bouchor published *les Chansons Joyeuses* (the Joyful Songs), a work full of freshness, of unconcern, overflowing with life. They are bacchic songs, love lyrics, poems celebrating the Goddess Nature, and the like. Christianity is cursed, for the young man is a thorough materialist. His Bible is Lucretius' poem; his god is science.

A few years later *les Poèmes de l'Amour et de la Mer* appeared. Sensuous love, strangely entwined with mystic aspirations, gives the tone to this volume.

In the meantime the reckless, unconcerned youth has met with the great problem of life. In the preface to *les Symboles*, he retraces his moral transformation: "Having understood," says he, "that the doctrine in which I saw the truth was devised to debase my mind and to narrow my heart, ... it became clear to me that *if I wanted to increase my intellectual pleasure*, nothing, not even virtue, should remain indifferent to me." Under the impulse of this desire he turned towards ideal justice, "but," continues our poet, "the good faith of my master[1] took hold of me, and I became transformed in contact with this sound and robust soul." The facts of moral consciousness had asserted their authority. In search of light he went back to the gospels, and understood better their spirit. The idea of God absorbed all his thoughts. At this time he perceived the limitation of science and its incapacity for satisfying his most imperative and noblest aspirations. Anguishing doubts beset him, which neither religion nor metaphysics was able to silence.

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[1] Proudhon.

During this long period of inward struggles, M. Bouchor writes his third book, *l'Aurore* (Dawn), in which he vents in passionate verses, often frightful in the intensity of their anguish, the torments of his soul. The true cause, or at least the chief cause, of his moral condition is revealed in the two first parts of the book, "la Chair (The Flesh), "la Lutte" (The Struggle). It is not essentially intellectual; it is sensual. The flesh, lust, devours him; in vain his soul, longing for purity, wrestles with his unbridled senses. We dare say that here is the cradle of the greatest part of French pessimism. In order to believe in an ideal world, the idea must triumph in oneself. When the flesh governs the mind, it becomes for it the only reality. Listen to him addressing his mistress:

> "Let me die with bliss in the enjoyment of the present,
> Eat my heart, drain my veins: again, do it again;
> Plunge me whole in an immense joy
> That in thy embrace, I may feel my soul die."

And elsewhere:

> "I want to clasp thee with shrieks of delight,
> And in a caress fit to wake up the dead,
> I want to encircle thee around my prey."

A little further:

> "Provided I see thee, touch thee, feel thee,
> All else is indifferent to me;
> The world is naught away from thy arms."

After satiety, regret and black despair seize him:

> "Nothing is left me but to wring my hands,
> And to cry as a child.—Courage I have lost.
> I am whirled, rolled, swept away by the storm
> As a dead leaf through the autumn fields."

At another place he exclaims:

"I do not love thee with my brain; it is the beast which adores thee; it is the maddened flesh, and my heart fails to silence my body."

The sorrow is as bitter as the passion is violent.

The last part of the book is calmer and more melancholy. He has fought so hard that he seems to have mastered his lustful desires. The poet thinks he perceives the dawn of a new day, and he pours himself out in mystical effusions, in which the love of the creature, the love of nature and the love of God are strangely blended; he hopes that "that uncertain, misty dawn will be followed by the light of a shining faith." But this hope was not realized, says he, in the preface already cited.

Where did the evolution of the poet stop? Here is, in his own words, the conclusion he reached:

"Religions express symbolically truths which language cannot directly utter, but these truths themselves appeared to me the far-removed images of a reality which I adored without knowing. The most ideal part of every belief was, in my eyes, as a veil which allowed the passage of but a scanty portion of the divine light; it is why, desiring to group in my book the greatest part of these pious reveries, I called it *les Symboles*.[1] . . . . After a fruitless search and certain deviations from the religious sentiment, weary of vacillating between contradictory systems, I came to a purely human and moral conclusion."

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[1] The name of Maurice Bouchor's last volume of poetry. He attempts in it to resuscitate the spirit of the antique religions.

Paul Bourget.—We feel embarrassed before this very complex nature. To give in a few minutes an adequate idea of one of the most subtle and complicated products of modern French civilization, in his various aspects and in his moral evolution, is no easy task. We beg your indulgence for this insufficient effort.

No man perhaps represents so completely as Paul Bourget the various tendencies, good and bad, and the peculiar psychic states which we have met with in the preceding notes. In him is found the synthesis of the perplexing *états d'âmes* of his generation. I desire to draw your attention specially on the gradual transformation which seems to have removed him from the side of the negatives to the side of the positives.

Twelve years ago Paul Bourget was unknown; today fame has carried his name in every civilized land. We find in him a poet: his first publications, *la Vie Inquiète* (Restless Life), *Edel*, *les Aveux* (The Confessions), are poetry; a critic: the *Essais de Psychologie Contemporaine*, 2 vols., and *Etudes et Portraits* have conquered for him a distinct place as a literary critic; a novelist. On his novels rests our author's fame among the great public; to the few who desire to see deeper into the fabric of his soul, the *Essais* and the *Etudes et Portraits* are Bourget's most interesting works. In them, in some passages of his earlier, and in many of his latter works, the reader finds himself in contact with a strong, disciplined and acute intelligence, with a philosopher seeking relations of cause and effect; while in his poetry, and in a considerable part of his novels, he appears as a mincing, feminine, elegant gentleman, very subtle and very sweet. A female sensibility and a male intelligence, is the first paradox which perplexes the reader. The knowledge of this duality may serve as a key to the understanding of his personality.

Love is his favorite theme, at least in the first part of his career. No one ever unraveled better the mysterious complexity of a feminine heart; the contradictions, the unconsciousness, the instinctiveness of fair humanity were never searched with so much acumen; no woman ever showed so much delicate refinement in the portraying of a heroine's toilet, of her boudoir, or of the furniture of a parlor.

The *Essais de Psychologie Contemporaine* is a collection of about a dozen *essais*, on as many men, whom the author conceives to have been the introducers of new forms of sensations and of thoughts. Notice the word "psychology" in the title. Bourget does not call himself a critic, but a psychologist, and this correctly, for he does not discuss talents or artistic theories; he analyzes minds. He does not depict and criticise; he attempts to set forth the internal processes which determine action, to unfold a soul, and to show its influence. The form, the exterior, draws his attention only in so far as it reveals the inner man. By the use he made of that psychological method, Paul Bourget became the leader of a new literary school called *l'école psychologue.* His purpose in these *Essais* is to show how the literature of to-day influences and moulds the ideas of the men of tomorrow. "My ambition," says he, in the preface, "has been to draw up some notes that will be of some use to the historian of the moral life during the second half of the nineteenth century." The author's point of view is subjective. He chooses the men who have been his initiators, and he sets forth mainly those special characteristics of each which have influenced him. But in thus studying those men from the point of view of his own experience, he really stands as the representative of a large portion of the cultured French of the present generation.

Since his endeavor in the *Essais* is to trace the influence of the literature of the middle of the century on the men of today, Bourget must take in consideration the laws of psychic life underlying the principles of ethics. I do not think it would be correct to represent him as concerned with morality because it is a question involved in the studies contained in the *Essais*, but rather the *Essais* owe their existence to his deep and constant sensibility to good and evil. I call your attention to this fact as one of the fundamental elements of Paul Bourget's personality. None of his books has yielded his deepest meaning if this be neglected in its interpretation. His novels are dramas of the conscience. His heroes wrestle consciously or unconsciously with the moral instincts. When A. France goes to the Imitation of Jesus Christ after reading *Mensonges*, it is to ease the intense sadness caused in him by the conflict of sense and conscience as depicted by the novelist. Exaggerating a little, one could say that the groans of a soul subjugated by the sensuous passions are heard arising from every page. In this Bourget is again a psychologist, for how can we conceive of a true psychologist who is not a moralist? Are not moral questions questions of life and death?

L'Irréparable is the story of a young girl who dies from shame and remorse after a pollution.

Cruelle Enigme sets forth the weakness of the spirit in conflict with the flesh; that is the cruel enigma.

The theme of *Crime d'Amour* (Love Crime) is the expiation and purification through moral sufferings, followed by reconciliation.

Mensonges relates the fatal *desillusion* of a young poet suddenly thrown, by his first success, into a luxurious and elegant society. It is the tragic conflict of the ideal with the reality.

These novels produce a painful and a depressing effect. A sensuous thrill and a longing for a spiritual ideal, simultaneously awakened in the reader, clash together and create a most painful emotional state. We conceive the author to be in the moral condition which his novels induce. Without ceasing to surrender himself again and again to the greedy claims of his refined senses, he cannot free himself from the besetting presence of his higher self. The ideal follows him. Hence the duality of his novels; hence their sadness and their pathos. The complaint of Bourget, as of all these tormented men; the complaint of *des Esseintes* of Lemaître, of Rod, of Bouchor, of Sully Prudhomme, etc., is more or less distinctly that of St. Paul. "For the good that I would, I do not, but the evil which I would not, that I do. O, wretched man that I am! who shall deliver me from the body of this death?"

Another fact worthy of attention is the little share that pure intellect has in Bourget's novels. Their author appears to move in the world of the feelings; he is a psychologist of the heart rather than of the head. His instrument of comprehension is *sympathy*. When he dissects an action, it is by means of the logic of the feelings. Facts exist for him only in their emotional concomitants. This is, indeed, one of the striking generalizations resulting from a study of modern French litterateurs: nothing interests them, nothing moves them, nothing is worth noticing, nothing exists for them save objects of feeling. Shall we not see in this phenomenon presented by an old sceptical civilization unfettered by convention or by tradition, the demonstration of the impotency of pure thought as a direct spring of action, and the fundamental importance of the feelings? The wonderful subtlety, the remarkable intelligence of the men we have reviewed, has its basis in an exquisitely delicate nervous system, reacting emotionally to facts which leave the vulgar unmoved.

In his first phase, Bourget is a negative, a sceptic, a dilettante, a pessimist. I quote from the *Essais*: "Only a prejudice in which reappear the antique doctrine of final cause and the belief in a definite purpose in the universe, can induce us to regard as natural and wholesome the amours of Daphne and Chloe in the dale, and as artificial and unwholesome the amours of a Baudelaire in the boudoir he describes." He admits the decaying state of France, but adds: "If the citizens of a decadence are inferior as workers for the grandeur of a country, are they not very superior as artists of the interior of the soul?" Notice, if you please, that this is said in a chapter on Baudelaire, with direct reference to that poet. A superior being is for him some one who discovers some new mode of thinking or of feeling. Ch. Baudelaire and E. Renan are both superior beings, because "by digging deep into their heart, they have invented two ways, until then unknown, of practicing, the first, debauchery, the second, dilettanteism. They have told their new dreams of the voluptuousness of the flesh and of the spirit in very bold pages, which have awakened in analogous and less personal souls tempting curiosities." There is here absolutely no thought of anything beyond art.

In the *Nouveaux Essais de Psychologie*, published later, moral scepticism seems to have lost ground. The mysticism already visible in some parts of his first studies is here more accentuated. He seeks a remedy to the "immortal nostalgia of the heart," and he perceives that the avowal of the heart's cravings is an open door on mysticisms, that it is the admission that there are intuitive truths which science cannot give us.

We have alluded two or three times to an evolution in the life of Paul Bourget. *Le Disciple* is perhaps the first clear indication of it. This novel, generally held to be the master-piece of Bourget, is the story of a young man who is led to perpetrate a crime by the logical deductions he made from the teachings of the determinist philosopher, Adrien Sixte. Its thesis is the responsibility of the teacher for the act of the pupil. It is a strong and pathetic appeal to the hoard of writers of all kinds who have so large a share in the education of youth. The author appears to have finally come to the full realizations of the vicious influence that such men as Renan, Taine, Baudelaire, Stendhal, etc., whom he revered and admired, have exercised on him, and, seized by the idea of the danger to which the young men, his brothers, are exposed, he throws out a passionate cry of warning. Listen to him addressing, in the preface, the young men to whom he dedicates his book. After having described two types of men, the one who at 25 years of age is a "calculating machine at the service of a machine for pleasure," the other very much alike to the author himself as he appears in his first works, he proceeds as follows: "Be neither one nor the other of these young men, thou, my brother! Let neither the pride of life nor the pride of intelligence make of you a cynic, a juggler with ideas! In our time of troubled conscience and of contradictory doctrines, cling to the word of Christ as to the saving branch; 'the tree must be judged by its fruit.' There is a reality of which you cannot doubt, for you possess it, you feel it, it is part of your life, it is your soul. Among the ideas which assail you, there are some which decrease the soul's power to love and to will. Hold for certain that those ideas are wrong in some particular, however subtle they may appear to you, however talented may be those of whom you receive them. Exalt in you these two great virtues, these two energies, outside of which there is nothing save present withering and final agony, *Love* and *Will*."

Unhappily the novel betrays the fact that the inspiring sentiments of the preface are, in part, only heart's desires, and have not yet become fast rooted in a character. Many a passage contained in it will not serve to exalt in the reader the two virtues without which there is but withering and ultimate death.

La Terre Promise (The Promised Land), published at the end of the past year, may be regarded as the first work of the second phase in the author's life. The dilettante palled with the dull realities of life; the sensualist moaning under the slavery of his passions; the sceptic playing indifference, has become a man of duty. He who delighted in the subtleties of the intellect, condemns now the refinements of elegance and the refinements of intelligence, as being incompatible with virtue. "Intelligence is negative; that is the brutal fact of which we must loyally acknowledge the certitude."

It might perhaps be said that the ethical worth of a man is measured by his views and habits respecting sexual relations, for sexual life is still more truly the centre of the moral than of the physical. Bourget, up to *la Terre Promise*, proceeded on the principle tacitly admitted by a large part of French society, that when a woman does not find in marriage the legitimate satisfaction of her heart, she is excusable if she gives herself to a man worthy of herself, in order to save from atrophy her best self. In that novel, Bourget breaks with this current morale. He perceives that he has been juggling on the brim of a precipice, and earnestly attempts to show the unavoidable degrading influence of all adultery, and the duties that such relations involve to the woman and to the possible offspring. *La Terre Promise* is the drama of paternity in adultery.

The skeleton of the novel is very simple. Ten years after having through ungrounded jealousy separated from his mistress, a married woman, Francis Nayrac meets a young girl of angelic purity, with whom he falls passionately in love. He has at last found the ideal for which he was longing. His suit is accepted, and they pass together with the mother of Henrietta, Madame Scilly, delicious days in a Sicilian residence. Not long before the date fixed for the marriage, chance brings his old mistress with a young child into his neighborhood. Through a striking resemblance to his own sister, Francis is convinced that the child is his. His bride discovers the secret, and an irrevocable separation follows. Soon after the mother of the child dies from consumption, and as Francis, all alone on the landing, watches the steamer which carries away his child to her mother's relative, it seems to him that at the extreme line of the horizon, colored by the rays of the setting sun, a luminous shore appeared as a land of light toward which the boat directed its course. It became for him the symbol of the new shore, of that other Promised Land toward which he was resolved to direct his walk. "The heroic sacrifice of the loving Henrietta had not been lost. The man of desire, of selfish emotion, the one who lived only to feel, even in disregard of the misery of others, was finishing to die off in him. . . . . He had the certitude that if she remained separated from him by her vow, she had at least rendered him the esteem of which he felt worthy now that he had become a man of responsibility and of conscience." His sufferings he accepts as a deserved punishment, and finds consolation in the words of Christ, "Take up my cross and follow me."

From first to last page the book breathes a spirit of sincerity and purity, a moral earnestness, in strong contrast with the dilettanteism of the preceding novels.

The psychic traits we have noticed in the preceding studies might perhaps be summed up under the following heads:—

(1) The thirst for sensations; voluptuousness.—The demands of an over-excited nervous system can no more be controlled by moral dictates. Sensitiveness to the beautiful characteristic of the French, facilitates the abnormal development of sensuousness, for, as Kraft Ebbing remarks, "In sensual love is gained that warmth of fancy without which a true creation of art is impossible." The normal equilibrium between sensual love and ideal love is broken.

(2) The spirit of analysis.—It is the instrument of the seeker after sensation. Such a person derives his enjoyment from the contemplation of his emotional and sensational states, and consequently is led to analyze his psychic states, in order to delight in their pleasurable contents. Moreover, the conflict which such an attitude induces in a being not utterly deprived of moral sense, draws the attention of the subject on himself and becomes an additional incentive to introspection.

(3) The absence of faith in the moral principles.—The facts of conscience, at all times present with the well-balanced man, have been crowded below the threshold of consciousness by sensuous and intellectual presentations. Dilettanteism is one of the fruits of this psychic state; it is the child of a non-moral being.

(4) Pessimism.—It cannot fail to accompany the absence of faith in a destiny. Revealed religion is an illusion; the promises of science are illusions; even free-will is an illusion; man is but a piece of machinery in an immense insensible mechanism, working we cannot know why; good and evil are empty words. Pessimism is, moreover, increased by the diminution of life consequent upon this depressing nihilism and upon sensual excesses.

(5) Painful longings for a vague ideal; mysticism and moral inertia.—Along with the preceding characteristics we find very often aspirations towards purity. This soaring upwards is in its first stage a religious mysticism uniting sensual with ideal love. This mysticism performs the function of a bridge between the "flesh" and the "spirit." It is highly interesting as showing the close connection existing between sexual and religious feelings. In the shifting emotional condition of the men in whom we have noticed mysticism, those two feelings supersede each other; although differing in quality, they appear to be inversely proportional quantities, so that we are led to think of them as different manifestations of a unique energy. A dismal moral inertia accompanies these first desires; the abuse in the contemplation of feelings has destroyed the power of action.

We might be accused by some of having drawn a picture of France darker than reality. If we had left the sphere of literature to go lower, among the books and publications of all sorts which have no claim to literary merit, to the "feuilletons" of many daily papers, to such reviews as *la Vie Populaire*, to the novels of Richebourg, du Boisgobey, Xavier de Montépin, Paul de Koch, Paul Féval, Gaboriau, La Comtesse Dash and others,—the moral rottenness of a portion of French society would have appeared much greater. It would, nevertheless, be a glaring mistake to suppose that the writers whose characters we have tried to delineate represent the totality of the French people. The comparisons sometimes

drawn between the Paris of to-day and the Rome of the emperors is the work of detractors or of misinformed persons. The vitality shown in the wonderfully rapid recovery from the defeat of 1870 would be sufficient proof of the falsity of such intimations. The current of life to which we have drawn your attention is very noisy, it is strong, and at one time threatened to carry away the mass of the nation; but there is by its side an enormous extent of dormant waters and also a counter-current steadily enlarging.

Before arriving at the end of our task, we have still to set before you the nature and extent of this counter-current, commonly named *Neo-Christianisme*. That shall be the object of our third lecture. But previously let us ascertain *the cause and the accelerating circumstances* of this movement.

Its true cause is found, it seems to us, in the moral and religious nature of man: an abnormal and consequently painful psychic state, as the one at the base of French pessimism, tends constantly, in virtue of its unnaturalness, to pass over into another psychic state, more in harmony with the fundamental needs of human nature.

A number of exterior circumstances have, during the past twenty years, come to the help of nature in stimulating the moral and religious aspirations. Among these accelerating circumstances we place the defeat of 1870, the Paris Exposition in 1889, Russian literature, and the influence of French Switzerland.

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The Defeat of 1870.—We begin with the first in date and the first in importance. The humiliated nation had too much spirit to lose heart. Vengeance should be taken. The wiser of the nation drew moral lessons from the unhappy war. Moralists and ardent patriots saw in the defeat the sign of degeneracy; they united to point out the root of the evil, and abroad went the idea that the nation had received a deserved punishment for its moral weaknesses. This idea accepted—and it was accepted more or less openly by many an influential man—you conceive what powerful impetus the movement of national regeneration received. Thoughtful men turned to a thorough and conscientious study of the causes of the reverse; comparisons were made between France and Germany, and out of the flood of tempestuous sentiments arose the steadfast purpose of achieving the salvation of the nation by what I shall term the moral reform. It took various names and various forms, but at bottom it is a moral reform, since the building up of men was the aim. The most important of them is the great reform of national education, concerning which we shall say a few words later; at present we simply desire to have you realize that the renovation has for one of its most potent elements the national sentiment aroused by the defeat of 1870. The *idée fixe* of the foremost statesmen and educators is the replacing of France on a footing of equality with its great neighbor and enemy, in order to be equal to any emergency and ultimately to efface the black record of 1870. This same idea lurks even around the basal notions of the less exclusively patriotic, the most universal of the leaders of the neo-Christian movement, M. Paul Desjardins. Listen to the impassioned words of the noble Ernest Lavisse, one of the most influential reformers of the national system of education, speaking to an assembly of students: "If I had not for our flag the cult of a pagan for his idol, which claims incense and at certain times hecatombs; if my heart was to forget our national sorrow, truly, I would no more know who I am and what I am doing in this world. I would have lost the principal reason for living." In the preface to *Etudes and Etudiants*, a collection of

speeches by Ernest Lavisse, we find these words: "They feel (the students) that our solicitude for them extends beyond the immediate object of their studies; that our relations are not only those of master to pupils; that through our lips a generation which has paid by public woes for its insufficiency and its faults, speaks to a generation that it desires to make better than itself in order to give to it at least an additional chance to be happy." Sometimes the wounded patriot forgets his moderation and such words as the following, uttered at a meeting of the Students' Association, carry the spur of vengeance into the hearts of his young auditors: "We, whose youth ended as the great national mourning began . . . . we would not die before having seen our France restored and avenged." You perceive the spirit. The life of M. Lavisse is completely devoted to the reconstruction of the nation through education. Raoul Frary, another educator, is animated by the same patriotic idea. In his book, *La Question du Grec et du Latin*, he writes: "The invasion of 1870 compelled us to confess an inferiority of which we had to seek the origin. The example of our victors themselves was an invitation to base on a better education of our youth the hope of retaliation, or the security of the diminished territory."

The well-known philosopher, Alfred Fouillée, begins his interesting work on *L'Enseignement au point de vue National* with these words: "A league for the *renaissance physique* has been founded in France and everybody feels that we no less need to unite to give birth to an intellectual and moral renaissance." Notice, if you please, the title of the book; since 1870, there is in France no other standpoint in education than the national standpoint.

When the minister of public instruction, Bourgeois, sends out instructions warning teachers to keep from their pupils all books with sceptical tendencies, all books that could tend to diminish activity, it is not that he has found a satisfactory basis for truth, but only that he realizes the dissolving action of doubt, and fears its consequences for the nation.

Let me remark here that the Papacy and the French patriots work here in the same direction, although their aim is not the same. It may well be that in order to achieve the end it has in view, the civil power will modify its attitude towards the church and welcome its coöperation.

It is the same patriotic feeling which has instigated the uplifting efforts of Melchior de Vogue. Read his beautiful articles published in the *Revue des Deux Mondes*, during the Paris Exhibition, under the title *A Travers l'Exposition*, and you will understand that here again the patriot makes the moralist.

This will suffice to show how powerful has been, and still is, the national sentiment aroused by the unhappy Franco-Prussian war in furthering moral reforms. It is, in truth, a morality of a low standard, that morality which is imposed as a necessary condition of the *revenche*; it is the morality of the athlete in training, who, in order to augment his chances of victory, curbs his bad passions. But it is, nevertheless, an element of progress to which due weight must be given; we must not forget that one of the strongest pillars supporting societies is made up of those blind and generally accounted unworthy passions, which could be designated as the passions of the struggle for life, rivalry, vanity and selfish ambition.

The very interesting question of the influence of the collective body, of the national instinct, on the individual finds valuable illustrations in the facts we have just laid before you. Allow me to digress from my subject for an instant. We distinguish two

sets of reasons why a national disaster should often appeal more strongly to the individual than his own moral abasement. On the one hand, the individual cannot be fully conscious of his own degradation, for that which in him judges, his conscience, is precisely that which changes. But when the body politic, when the fatherland is suddenly crushed down, the citizen remains able to perceive in that objective fact a sign of degeneration. Moreover, while in the case of personal looseness of life, our will is hampered in its efforts at reform by the alluring sensual pleasures attached to many evil deeds, no such counteracting influence fetters our activity when the nation and not ourselves is the victim. The powerful and deeply ingrained sentiment of rivalry is also strongly awakened by a national reverse. There is an enemy before us; it is not a mere intangible passion; it is a collection of men like us; where is the valorous man whose powers of life are not quickened by the thought of a human adversary?

On the other hand, the altruistic instincts receive a strong impetus by a national danger. It is no more a question regarding ourselves only, or, perchance, our family, but it is a menace to the community with which we are bound by all sorts of ties, and further, to the whole nation, whose survival we recognize to be of more importance than our own, for we cannot escape the influence of the number.

Russian Literature.—About 1884, the sympathetic and eloquent pen of M. de Vogüé introduced the Russian novelists to the French public in a series of articles published in the *Revue des Deux Mondes.* A few of their books were translated and met with a prodigious success. Within a few years Russian literature invaded, we might say conquered, France. It was a craze. Gogol, Tourguenef, Dowtoiesky, Tolstoi, became the subject of gossip for society ladies, and of discussions for the men of letters. There is something almost mysterious in that subjugation of highly cultured France by the half barbarous country of the Czars.

What were those bewitching ideas coming from dark Russia? What could come from sensitive men in close touch with modern knowledge and western civilization, witnessing, as fellow-countrymen, the social barbarity and the wide physical and moral misery of their kinsmen; what could be in the breast of such men but sadness, revolt against civil power,—perhaps against divine power,—pity and despair or, perchance, hope?

Russian literature is pervaded by a strain of intense sadness and a deep pity. But beyond the sorrows of human existence, the writers see hope, for, however radical the intellectual scepticism of some of them may be, conscience and duty remain undethroned. Their books throb with moral earnestness, and whatever pessimistic expressions may be found in their pages, the reader leaves them with the impression that man is not a mere waif tossed nowhere by the billows of life, but that he is rolled about in a world of sorrow by his own passions. Sin is the cause of the sufferings of man.

Russian novelists have captivated the French, because they treat of the life and death problem, which, unable to solve, the French appeared ready to abandon: the meaning of life, because they bring to its study the sympathy of a young healthy person, instead of the hopeless sorrow of a decadence; because they have a true pity, or better, a Christian charity for the sufferers and the fallen; because they reveal a world of faith and of action.

Sincere emotion, unselfish interest in others was blunted in France; the strong, vibrating voice of the Russians has vivified the

source of human sympathy and of brotherly love. Compassion and pity are the two virtues which have penetrated deepest. It was, perhaps, the best gift that could be made to France in a time of cold indifference and of monstrous development of egoistical individualism.

The Paris Exhibition in 1889 must be counted as one of the events which greatly stimulated the various reforms. The French never lost confidence in themselves, but the proof of vitality, the vindication of the immense resources of the nation, of the industry, skill and intelligence of the people which the great exhibition furnished, filled every French man with a new hope and a new energy. In 1889 France resumed, in its own opinion, its former place among the great powers of Europe. The exhibition was the first visible step towards *la revenche*; it became the voucher of a new era.

French Switzerland.—The exchange of thoughts between French Switzerland and France is considerable, for the language is the same in the two nations and they are contiguous. The Suisse Romande unites many of the traits of the Germanic to many of those of the Latin race. The French Swiss is less brilliant, but perhaps more reliable, steadier; less enthusiastic, but more persistent; coarser in his sensations, less artistic, but morally stronger than the French. Thanks, in part to its vital Protestantism, the morals of Switzerland are comparatively pure and simple. Its literature reflects the national character, and exercises in France an influence often disregarded by its great neighbor out of a sentiment of ungenerous pride.

The philosopher who is looked upon as the philosopher of the neo-Christian movement now in progress, *Charles Secrétan*, is a Swiss professor. Paul Desjardins could aptly be called his pupil. Long practically ignored in France, he has come into prominence with the religious awakening. At the beginning of the year, Desjardins and his friends induced him to come to Paris and meet the French philosophers in public discussions in the interest of Christian spiritualism. The students seized upon the occasion to manifest the honor in which they hold the venerable professor of Lausanne University. We shall have a few additional words to say on Charles Secrétan at a later time.

Brunetière, the first French critic of the day, recently elected member of the French Academy, is one of the very few litterateurs of mark who hold to a positive criterion of truth. In an article on *Alexandre Vinet*, the Swiss critic and theologian of the middle of this century, he declares that he owes to no historian of literature so much as to the author of the *Etudes sur Pascal*. If Brunetière, in opposition to nearly all literary France, approves of Vinet putting the ethical question to the front in a history of literature, who shall say that he does not owe that high notion to Vinet himself, with whom the moral value of ideas makes the value of literature? The same authority, speaking of the *Etudes sur Pascal*, says: "It is the most exact, the most penetrating and the deepest work ever written on the author of the *Pensées*. When Sainte Beuve was composing his masterly history of Port Royal, he went to Lausame and spent there a year in close intellectual companionship with Vinet, whom he venerated as a master.

We need not revert here to Edouard Rod, whose spirit we have tried to set forth when speaking of the Tormented, nor do we need to dwell longer on this topic. What precedes will suffice to indicate that the influence of French Switzerland is in the direction of the moral and religious revival going on in France.

Transformation in the Roman Catholic Church.—We cannot pass on to the neo-Christian movement without taking cognizance of the present attitude of the Roman Catholic Church with respect to France. Under the far-seeing leadership of Pope Leo XIII., the church is undergoing an epoch-making transformation, aiming at a better adaptation to the social and scientific progress of the world.

Among the many history-making acts of Leo XIII., there are two of special interest to us: the return to the philosophy of Saint Thomas Aquinas, and the acknowledgment of the Republic. In the encyclical letter, *Æterni Patris* (1879), the Pope recommended the study of Saint Thomas as the best philosophy for Catholicism. A little later an academy was founded at Rome to set the example. Not long ago, a baccalaureate in scholastic philosophy was established at the Catholic Institute of Paris. Nearly every week sees new books on Saint Thomas. At the order and expense of the Pope, a new complete edition of his works is being issued. Discussions and comments on his doctrines fill the Catholic papers, and this not only in France, but still more so in Germany, in Belgium and in the United States. Since Leo uttered this encomium—"Reason carried on the wings of Saint Thomas to the pinnacle of human nature can hardly rise higher"—the name of Saint Thomas is on the lips of all the faithful. "Thus, says Saint Thomas," is now the watchword in the Roman Church.

What is the significance of this move to the peripatetic philosophy? Two motives are attributed to Leo XIII.: The opposition of the church to the republican form of government had become dangerous to its existence in France, and was a serious impediment to its progress in the United States. The doctrine of Saint Thomas allows of all sorts of governments, according to circumstances, on the principle of the natural rights of man. So that the acceptance of his philosophy rendered legitimate the step which the Sovereign Pontiff contemplated taking, the adhesion to the French republic. The other motive is to make possible a Roman Catholic unanimity in philosophy, for if the Catholics have been united on the questions of dogma, their philosophical tenets have greatly varied. The political successes of the Catholic party in Belgium and in Germany are due, in part at least, to the spirit of unity produced by the new papal policy.

Another very significant sign of the new life which is being instilled in the church by the powerful breath of the venerable Leo XIII. are the Catholic congresses of scientists. "Science is our great enemy; then let us make it an ally by developing among us the scientific spirit and scientific knowledge," thus spoke certain well-advised Romanists. The first congress, a very small assembly of men of little notoriety, met in 1888; the second was held in 1891, and proved completely successful; a third one is announced for 1894. From the first the warm sympathy and active support of the Pope assured the success of this remarkable association. The Holy Catholic Church enticing young men to seek in the field of science the renovation of its apologetics! It is, indeed, a wide departure from its traditional policy. One can well ask whether science and scholastic philosophy, united by the paternal hand of Rome, will live together peaceably. These congresses of scientists have already produced such fruits that one of the redactors of the *Revue Philosophique* has said: "We shall soon have to reckon with the Catholics in the religious, social and pedagogical questions. France will soon know, as already Germany and Belgium do, how much vitality the return to Thomism has infused into the Roman Catholic Church."

In *résumé*, it appears that the Roman Church is undergoing a transformation in order to adapt itself to the changed condition of society. It had remained too far behind modern civilization; now the wise Leo XIII. is making a powerful effort to regain contact with the modern world. How far will this liberal tendency go? Will it, under the influence of such men as Desjardins and de Vogüé, extend to the essential reforms, which alone will permit of its reaping the fruits of the spiritual awakening? If the Catholic Church is to draw to its bosom the French youth, the reforms undertaken are only precursory to much deeper transformations. But we shall have occasion to recur to this point when speaking of M. Desjardins.

III. The Neo-Christian Movement.

Ernest Lavisse, Melchior de Vogüé and Paul Desjardins, around whom the forces of moral reconstruction centre, differ widely from each other, although generally mentioned together. All three have at heart the moral renovation of France, but their conception of the evil, their attitude before the problem, and the solution they propose, separate them sharply from each other.

Their field of direct activity is the cultivated youth and especially the Paris' students. Lavisse and Desjardins are both professors at the Sorbonne, and, although not in official connection with the students, de Vogüé has become by his writings their admired master. All three are literary men; Desjardins is professor of rhetoric; Lavisse occupies the chair of history, and de Vogüé is one of the most brilliant writers of our time. The two last named are members of the French Academy. We note the exceptional fact that this moral and spiritual awakening starts from the higher sphere of intellectual and social life, while heretofore the religious conquests have generally been made with the help of ignorance, or at least independently of learning. We welcome this fact as a sign of true progress. Christ's message was primarily addressed to the poor and the lowly; it finds more ready acceptance among those whom neither wealth nor social position nor intellectual ambition incites to unrighteous living. Today the learned and the great become the prophets of the revival. It is the vindication of intelligence, it is the glorification of the democratic spirit, it is also a sign of the recognition that the evil lies in great part in a false exercise of the mental faculties.

We shall endeavor to represent to you the spirit and the activity of these three persons.

ERNEST LAVISSE.

Ernest Lavisse wields the greatest influence over the university students. He has gained their confidence and their admiration; he is the beloved master to whose enthusiastic and authoritative voice they intently listen. Far from confining himself to his historical teachings, his large mind and his patriotic heart are chiefly concerned with the reform of education, through which it is hoped France will regain the preponderant position lost in 1870. According to his belief, universities create nations. Lavisse is not moved, as is Desjardins, by the degradation of the individual. We have had occasion while speaking of the circumstances which stimulated the moral movement to quote a few passages from our historian. From them you have gathered that the moral evil appeals to him through the degeneracy of the nation. If he seeks with all the might of his powerful nature to rebuild manhood, it is not out

of hatred for sin, nor chiefly out of love for his fellow-men, but to the glory of his beloved fatherland. Before him stands constantly the spectre of France vanquished and threatened with complete destruction by the foe of the east. Under the spur of this vision he buckles on his armor and calls upon the young men to prepare for the coming struggle. The following passage from a speech to the students of the Faculté des Lettres reveals why Ernest Lavisse is a historian, and at the same time why he is one of the most ardent advocates of the reform of national education: "I follow an intimate and very urgent sentiment when I insist on the necessity of a serious and sustained effort in the study of our own history. This sentiment is that the University of France has certain duties to fulfill towards your country, duties resting especially on the professors of history. My conscience would reproach me if I did not represent to you that in a time when the rivalry among the nations is violent and will become ferocious, every people must richly nurture all the sources of its national energy. . . . . . To-day the most active nations seek in their origins the demonstration of the rationale of their existence, and seek in the past the guarantee of their future. . . . . Then, either we must absolutely deny the power of ideas and of sentiments on the souls, and consequently on the activity of men, or we must admit that the national energy is increased when a people is given the consciousness of its value, and a feeling of pride in its history; when the enlightened men have a clear notion of the genius of their country and of the *rôle* it has played in the world, and when a sentiment of pity towards the fatherland descends from the high regions of historical researches down to the deep stratum of the people."

The written work of Lavisse is not considerable. He exercises his influence and accomplishes his purpose through daily contact with the students, through speeches and allocutions delivered in their assemblies, at banquets, at the opening of the sessions of the Sorbonne, and on every other favorable occasion. Some of his speeches and discourses have been collected in two volumes, *Etudes et Etudiants* and *Questions d'Enseignement National.* He published in addition two historical works, *Vue Génèrales de l'Histoire politique de l'Europe* and *Trois Empereurs d'Allemagne.* Often he addresses his inspiring words to the teachers and the professors, laying on them the responsible duty of forming citizens by whom France's future grandeur may be edified: "We have to-day many things to say to our youth, things which are not in the curricula nor in the examinations. We must not make an abuse of those confidences, of these advices, of those high moralizings, but we must not neglect that part of our duty. It is a very great error to leave the essential untold under the pretext that it is understood."[1] Similar words are uttered by other eminent professors. In his book on *L'Education dans l'University*, M. H. Marion writes: "Do not fear to go down from your chairs among your students; do not fear, between two explanations of Latin texts, between two corrections of exercises, to warn, to direct, the consciences of those over whom you are the guardians, and who expect, who demand, from you something else than mere notions and classification of knowledge."

One of the signs of the new life pervading the French students in which Lavisse lays great hopes and great pride is the *Students' Association.* Prior to 1884, the students of the different parts of the

[1] From the preface to *Etudes et Eutdiants.*

University of France had no relations with each other. At that date the Paris association was authorized. In 1889 it numbered 1,550 active members, with an endowment fund of 13,000 francs, and about as much available money. Since then the society has greatly increased in number and activity. The purpose of the association is simply the concentration of the French youth, the union of the various schools in order to create an *esprit de corps,* to stimulate and to help each other. It is in his relation to the association that the large-hearted sympathy of Lavisse and his warm patriotism best display themselves. No occasion is lost by him to fraternize with the students, to rejoice or to mourn with them, to warn or to praise them, and above all, to breathe into them the burning patriotic spirit which is his life. He comes to the tomb of one of their number to weep with them and to comfort them. Addressing himself to the dead, he says: "My dear Delambre, as to me I was not satisfied to love you. Deep in my heart I felt for you sentiments of gratitude because you possessed the qualities and the virtues which we wish for the French youth; we whose youth ended as the great mourning began, we would not die before having seen our France restored and avenged. Your name remains associated with that sacred hope."

The following citation gives a good idea of the tone of Lavisse's patriotic speeches, and indicates the importance attached to the Students' Association. It is taken from an eloquent address to the students after the return of the association's delegates from the festival of the University of Bologna in 1887, at which the principal universities of every civilized nation were represented: "I feel still the charm and the gracefulness of the festivals of hospitable Bologna. But above all else I admired there two things, the enthusiastic salute of our Italian comrades to our flag, and your serious and proud manner of holding that flag. . . . . The return from Bologna has been the occasion for an ovation, for the public begin to understand that the students are able to do a national work. You have at last given to French youths their legitimate place. They were an anonymous crowd, disseminated in the faculties and in the schools, knowing not each other. You have made of them a body, noble among all, a person in the nation. Through you we know what *les jeunes* are. Formerly we could have believed that *les jeunes* were a few original young men, mannerists, dilettante, or worse than that, men who carry with them the disgust of life as a new fad. *Les jeunes* are you, you whom we have seen vibrate at the sound of certain utterances, quiver with certain emotions; whom we have heard express by acclamations addressed to the chief of the state the cult you profess for liberty, for honor and for your country. You are *les jeunes*, you, my friends, you who sing, you who laugh, you who work joyfully. You possess activity, valor, common sense, gaiety, humor, enthusiasm; you possess the soul, the whole soul of France. I am among those to whom you have more than once during these last days brought tears to the eyes. I thank you for it. We who have suffered much regain confidence in the perpetuity of the renovation of the national forces. We see after our winter the herald of a bright coming spring."

Where is the young heart that would not be carried away by so much sympathy and by so noble and so deep emotion?

MELCHIOR DE VOGÜÉ.

The Vicomte Melchior de Vogüé, the youngest of the forty Immortels, is a talented gentleman of leisure, who, after having

served his country in the unhappy war of 1870, has set himself quietly to philosophizing on the state of France. Having understood and felt the contradictory sentiments which agitate the young generation, its aspirations, its moral sufferings, he has sought the solution of the problem.

M. de Vogüé is one of those rare, harmoniously developed minds, so full of imagination and of elegance, so bright and so easy, that their prose is comparable to the murmuring brook winding its elegant curves across the flowery meadow. As far as we know, he never wrote a real book, only short compositions, appearing generally in the *Revue des Deux Mondes*, have come from his pen. Some ten years ago, he introduced the Russian novelist into France. On that score only, France owes him a heavy debt of gratitude. It would be difficult to estimate how much the literature of the country of the Czars has modified him. His literary baggage comprises a series of articles on the great Russian writers; studies on his favorite authors, Lamartine and Chateaubriand, whose brilliancy of style is often equaled by de Vogüé; historical, or rather historical-poetical reveries; a few symbolic and mystic novelettes; a number of articles on the Paris Exposition, etc. Many of these writings have been united in book form under the titles: *A travers l'Exposition*, *Heures d'Histoire*, *Spectacles Contemporains*, *Regards Historiques et Littéraires*.

M. de Vogüé is a poet, although he writes but in prose. We regret that he is so strongly inclined to poetical beauty, to art and to mysticism. His *rôle* of moral director of his generation is thereby greatly diminished, for all along the road he stops to gather honey from the flowers, and often forgets practical reality to lose himself in beautiful fancies. At such times those who would like to follow him doubt his earnestness and are tempted to distrust their guide. He has neither the enthusiasm, nor the bottom-deep convictions, nor the hatred of moral evil, nor the boldness of a true reformer. French youth has for a while gathered around his enchanting and sympathetic voice; it has bewitched them, it will not be able to incite them to a regenerating activity.

But we have not yet said what are the essential opinions of Vogüé as a reformer. He believes in democracy and science, those two queens of the age; but science and democracy have not the full secret of life. Of themselves they are unable to maintain social life. The principle which will save the nation is the old leaven of the gospel — love, and the spirit of sacrifice which accompanies it. Science and democracy must recognize that mysterious principle. While visiting the Paris Exhibition he stopped one day before the "Declaration of the Rights of Man," that proclamation of the principles promulgated by the Revolution, and to this day the official basis of the French state, which was written in large letters on the walls of the school exhibit, "My country and my century," says he, "appeared to me as coming out of that fatidical placard. . . . . I read twenty times each line, sincerely endeavoring to find a solid foundation for the support of that enormous weight, the social life of a great nation, and every time I came back to the same conclusions, all that I read on that wall is beautiful, generous, desirable, but it is a dream," and one by one he takes the affirmations or the assumptions of the Declaration and judges them false or incomplete: the law is not the expression of the general will; we are not born good; we are not born free and equal, etc. Our ancestors of the great Revolution moved in a chimerical world. "The nothingness of our social foundation

appeared fully only after one hundred years of consecutive destruction; a hundred years during which our France has staggered from convulsion to convulsion from the lack of a solid footing on which to steady its course. . . . . The whole century runs on that chimerical ground, and people wonder that it totters."

Thus far it is very well; the science-intoxicated youth feel now too well the insufficiency of the principles of the Revolution to resent de Vogüé's impeachment of this most glorious historical event. But he goes further. After having said the inadequacy of science and reason, and named its complement, love and sacrifice, he points out to Rome as to the power by which the desired renovation can alone be accomplished. Not long ago in an article entitled *Pensées d'Histoire dans Rome*,[1] he boldly exposes his views in this respect. Before the suggestive ruins of past ages, our historian is deeply impressed with the idea that we are on the wrong track with our analytical rage, with our confidence in the detailed document, with our pretension to explain life by laboratory dissections. "The coming world pants for recomposition; it will be grouped only around simple ideas." Now these simple ideas, destined to crystallize the coming era, are found, according to de Vogüé, in the Roman Catholic Church. Rome is the centre of history. It is to the papal Rome, from which we appear ready to break loose, that we must return to escape social death. There the infinite chain of events which make up history begins and ends. It would be highly entertaining, if so serious a question admitted of mirth, to observe the sensitive imagination of our poet carried away over every obstacle before the expressive remnants of the past grandeur of Rome. He tells us that the column of Trajan, terminated and dominated by the statue of St. Peter, a nimbus around the forehead, the keys in the hand, will always be the centre of the world. His veneration for the Pope is no doubt deserved, but many will refrain from joining him in his submissive admiration of the *Vigie*, as he designates him, "in the tower of the Vatican, seeking the road for the world committed to his keeping." M. de Vogüé does not know exactly how the nineteenth century's link will manage to get in the chain; but that does not seem to disturb him in the least, for he assumes the attitude of a true Romanist: the Vigie it is who shall find out the way; as for him, his duty is fulfilled when he has pointed to the astonished youth the new brazen serpent to which the universe must look to be saved. The questions of infallibility, of sacerdotal hierarchy, of theocracy, of the various dogmas, sacramental and others, he does not even mention. Is it that he fully agrees with all the doctrines of the church? We can answer no without hesitation. Somewhere he deplores that orthodox teachings do not persuade French youth; it would be otherwise, says he, if those teachings claimed the best established among the scientific doctrines in vogue, if they showed how with a transposition of words determinism becomes the doctrine of grace and predestination; how heredity, with all its biological consequences, enters into the conception of original sin; how selection becomes the redemption by works, etc. But we may suppose without great danger of being mistaken that the Roman Church is not quite ready to make the suggested transposition of words, and meanwhile we do not see well how M. de Vogüé can get along with the church. Let us remember—and this will perhaps be the key to a great puzzle—that beauty is the supreme argument to an artist, that it is abundantly able to cover a multitude of false notions and super-

[1] *Revue des Deux Mondes*, 1892.

stitious ideas, and that the Roman Catholic Church, in its history as well as in its actual form, has a great deal that appeals strongly to an æsthetic nature. His blind veneration for the Church of Rome becomes offensive when he declares that only the men who wear the cloth and who have acquired the right of commanding the hearts through the bruises inflicted to theirs by the triple vow of obedience, chastity and poverty, have really the right to carry on a religious reformation. "Let us be content," says he, "to be approximately honest people, that is already not so easy, even with the seven sins a day conceded to the wise." "The great blow of holy folly which is to change the world, if it must come, will be dealt—it is at least probable—by one of those men who are the natural ministers of the sublime follies, again by the right of their cloth and of their triple vow." The right of their cloth! That sounds decidedly too clerical to please the youth of the nineteenth century. M. de Vogüé might learn before long that the spirit of sublime folly does not blow only in the tattered garments of the traditional church.

PAUL DESJARDINS.

We have come to a personality very different from that of Lavisse or of de Vogüé. The sweetly ironic critic whom we have had occasion to quote, Anatole France, draws the following portrait of our professor of rhetoric: "As to M. Desjardins, one cannot reproach him with a too frivolous gaiety. I do not think that I shall displease him if I say that he gives himself the face of an apostle rather than that of a critic. He is severe. He does not like that people should write. To him literature is the beast of the Apocalypse. A well turned sentence is a public danger. He does not criticise, he anathematizes without hatred. Pale and melancholy, he goes about scattering tender maledictions." Even so would France have spoken of many a holy man whose name is kept in the calendar of the church. To be accurate, the portrait should describe him as "deeply serious" instead of "melancholy," and the word "literature" should be understood to mean that French modern literature of which we have spoken at the beginning.

With M. Desjardins the loud declamation of the natural rights of man has changed into an appeal to the natural duties of all men to their fellow-men; the love of the country, patriotism, has been replaced by the larger moving force, the love of man — the all embracing love; and Roman Catholicism by Christianity. "Thou shalt love thy neighbor as thyself" is the key-note of his message. We have recently re-read the few pages we have from M. Desjardins, together with scattered talks of others about him. From these perusals we have received one of the most Christ-like impressions that ever man made upon us. We have discovered no reason for restriction of approval or of admiration. There are today many a youth and many a man of high social position proud of being called his disciple.

Before proceeding we would like to prevent a possible misunderstanding which the appellation "neo-Christian" might cause. What is being revived contains nothing new, it is simply Christianity devoid of the adulterating additions made by the apostles and after them by the church.

To understand and estimate correctly the significance and the probable consequences of Desjardins' work, it is to be kept in mind that he does not speak from the desert or from some consecrated asylum of devotion; on the contrary his voice is that of one fed

with all the knowledge of the age; it echoes in the halls of the old University of France, the centre of this tottering civilization; the same halls which only the other day heard the sarcastic paradoxes of Renan about God and virtue, and the despairing stoicism of Taine. His disciples are not the disinherited and the ignorant, but the members of the University; the vehicle of the new propagandism is not some modest church paper, it is the great political paper, *Le Journal des Débats*, or the literary review, *La Revue Bleue*, or even the *Figaro*. But let us pass on to his two most important publications, *Le Devoir Présent*, and *La Conversion de l'Eglise.*

Le Devoir Présent, Paris, 1892. This small pamphlet is the manifesto of a man burdened with a mission of moral reform. *Primum vivere* is its motto. The preliminary step in an ethical reform is to come to a decision as to whether the subjection to animal instinct, to egoism, to lying is an absolute evil, or if it is only an inelegance. This debated question is surely more important and especially more urgent than that of the divinity of Christ, or even than that of the existence of a personal God. The author has settled that question for himself. "I profess, in all certitude," says he, "that humanity has a destiny and that we live for something." He does not know exactly what is to be understood by the word "destiny." "Thereupon I have only dreams born of a deep but incommunicable love which an equal love only could understand." In the battle fought around these great questions the negatives appear to have the upper hand even without any hope of reversal. The liking for duty seems decidedly to have passed away. Voluptuousness in all its forms, sensualism, is the plague which devours our society. "If one wants to understand what fiery vice burns in us, let him simply observe the looks that dignified men, gray-headed men, cast on an honest woman passing by. What tension, what spasm of lustfulness!" France has lost its soul and it struggles to regain it. We know well what is meant by soul. The humblest among us has felt at certain times superior to himself, he has been filled with the spirit of sacrifice that is, in reality, with the spirit of liberty. We have all observed the fluctuations of that soul in us, now arousing us to enthusiastic activity, now leaving us cold and passive. M. Desjardins does not know how that sublime state of waking love develops, but he knows that only such a state deserves the name of positive morality. There have been times when such a spirit inspired and moved France: in the twelfth and thirteenth centuries, *e. g.*, at the time of the Crusades. The Crusades were the proud victory of a society on natural egoism. Shall such a period recur? "I answer intrepidly that I so believe." Then the author mentions some statements which tend to render to national life a little of the altruistic energy it has lost; the territorial acquisition in Africa is a gift of hope, for it will call for energy, patriotism, sacrifice; the question of Alsace-Lorraine, which is for the French similar to that of the Irish question for England. With the reawakening of the national life, he believes that infallibly the moral and the religious life will be stimulated by reason of their solidarity. Our prophet has sometimes answers which do not satisfy reason, but they are given with so much confidence that they almost persuade. Do you know why France is about to recover her soul? "The sure answer is, that to live one must have a soul. We are then at the eve of having a soul. Let us hold to that position; it is very strong." The other alternative, death, does not seem even to come to his mind. Now since it is admitted and recognized that the hour is approaching at which humanity will recover possession of itself and resume

its ascending march, we have to hasten with all our might the arrival of this happy moment. The future shall be what we now will it to be. "It is why at this decisive instant, when I am about to expose my plans and mark my foot on a virgin snow, I cannot repress the joy, the divine ardor which penetrates me." For years the author had caressed in secret the hopes he now expresses publicly.

The first thing to be done is to understand each other, we positives, to unite ourselves for concerted action. We hear all around us that on some religious creed only we will be able to unite efficiently; as if man could not live until he has made a theological or philosophical stage. Our work shall have in no way an ecclesiastical character. The divers faiths which express themselves socially by the same acts and by an equal love, whatever they may be called, are for us synonymous. Our position is at the confluent of the multifarious sources of morality and of the good desires called Catholic, Protestant, Jewish or philosophic. Everyone while uniting with us may keep his special faith. Our only demand on our associates is that they should live for something, that they should believe in a duty. Here he quotes M. Secrétan, "The cause that we would like to serve, the crisis which our prayer asks, is not a return to the past, but the advent of a new era; it is Christianity in spirit and in truth, which has always subsisted in some souls, but has never reigned." The new faiths proposed, neo-Catholicism, neo-Protestantism, neo-Buddhism, are equally inefficacious, for they reach the heart and the will only through the intermediary of intelligence; they are speculative. The question is not to believe, but first of all to love. Abandoning all project of union on such or such a speculative truth, *we want to reach faith through obedience to duty.*

The last pages of *le Devoir Présent* are taken up by an enumeration in thirteen points of the practical fields in which for two or three years the activity of the positives should be directed.[1] It seems that after this preparatory stage, M. Desjardins contemplates undertaking a more aggressive and sharply limited work of moral reform, such as experience and circumstances shall indicate.

We have said elsewhere that M. Desjardins could be called a pupil of M. Charles Secrétan; as we have already consumed too much of your time with the matter here brought before you, we shall abstain from dwelling lengthily on this philosopher, whose influence is becoming preponderant, in France and in Switzerland, in the spheres of religion and of sociology. But we shall avail ourselves of this occasion to indicate the leading ideas of his philosophy, in order to give honor to whom honor is due, and to mark still more clearly the character and scope of the neo-Christian movement.

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Charles Secrétan, professor at the Lausanne University, Switzerland, correspondent of the French Institute and of the American Association for the Advancement of the Social Science, can be fitly designated as the philosopher of free-will. His system of philosophy finds its strongest support in the *facts of conscience;* he endeavors to reinstate them in their proper place, from which they seem to have been removed by the theory of evolution. For him religion is a form *sui generis* of the moral life; it is not, as many think, a childish form of science; it must necessarily subsist as

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[1] For complementary information on *le Devoir Présent*, see the *Pedagogical Seminary*, Vol. II. No. 2, p. 258.

long as humanity itself, through all the phases of its development. In *la Civilisation* and *la Croyance*, in which he reviews and gives an answer to the great social, theological and metaphysical questions from the practical point of view, he declares that "the supremacy of the moral idea is the vital element of modern thought, that it is the seed of truth which must, before all things else, be preserved and cultivated," and also that "to think of oneself, to live for one's own self, is to lose one's life. To give ourselves to others is the only chance of salvation: such is the lesson which the present circumstances teach us, ... that truth of the present moment is the eternal truth, it is the whole truth . . . ."[1] It is not amiss that such a philosophy is called Christian. Elsewhere we find these lines pointing again to the centre of his philosophy: "In a time when all the artificial props are ruined, ... in a time when moral checks alone subsist, when all depends more manifestly than ever on the individual will, to redress that will, to state precisely the idea of duty, to reanimate the sense of duty, in putting it in its place in the centre of life and of thought,—such is the true question, such is the object of our effort." The appeal to the facts of moral consciousness is the point of departure and the bulwark of the whole system. The way in which these facts are presented as scientific facts deserving preëminence over all the other facts of consciousness, and the confidence with which it is done, make of M. Secrétan's work an original system of the highest common sense philosophy. His published works are numerous: *La Philosophie de la Liberté*, 2 vols., *le Principe de la Morale*, *Recherche de la Methode*, *Raison et Christianisme*, *les Droits de l'Humanité*, a volume on *Victor Cousin* and one on *Leibnitz*, *la Civilisation et la Croyance*, etc.

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We come back to M. Desjardins. Until 1892 he had preached the need of a reform, and in *le Devoir Présent* had traced the lines along which it should be made. In January 1892, he went a step further toward the realization of his theories, and founded the *Ligue pour l'Action Morale*. The earnestness of M. Desjardins is manifested by this decisive step. The *Ligue* is composed of a mere handful of men, but in such an undertaking number is not often a token of strength. It works in silence and almost in retirement. We surmise that for the present, in the face of the difficult task before them, these men feel the need of meditation; they await the coming of the inspiring spirit. The members of the *Ligue* belong to the most varied faiths and social positions; men allied to no church, heretics as to all religious confessions, Roman Catholics, Protestant pastors and laymen are united in the same desire and in the belief—for them the most and perhaps the only important articles of the various Christian faiths—in the categoric imperative of duty and in the power of love. The *Ligue* publishes every three weeks a bulletin for its members.

At times when we allow our imagination to stray away in the beautiful ideal world, we see this humble association rising in strength and in spiritual influence, until its ramifications spread all over France, transforming the bitter fruits of incomplete civilization with that old leaven of Christian love which once already, 2,000 years ago, renovated the world. Is that only a dream? Shall it not find realization? The occurrences of the present seem to give an affirmative answer to this last query. Religions of intellectual

[1] The reality and the extent of the corruption which we have tried to set forth in our rapid survey of modern French literature, find additional confirmation from the fear of social ruin openly expressed by such men as Secrétan, Lavisse, Desjardins, de Vogüé, etc.

creeds are yielding place to the universal religion of the heart; intellectual barriers are already falling to pieces, not everywhere, nor completely, it is true; were we to forget it, we would quickly be recalled to the true state of things by one of the many for whom to be Christian is to believe in a certain set of doctrines and dogmas. One of the standard-bearers of French militant Protestantism, writing in the *Revue Chrétienne*, a strong organ of advanced Protestantism, scoffs at Desjardins and at the *Ligue pour l'Action Morale* for putting action, not only before faith, but even above faith. "A Christian," says M. de Presseusé, "has the duty to salute, with sympathy, the first timid stammerings of a moral reform; he has not the right to associate with such an enterprise on the ill-defined ground of a kind of religious neutrality. . . . The harbor is no more to be found, it is at the foot of the Cross." It is precisely the foot of the Cross—to use an illustration distasteful on account of its many associations with bigoted ideas—which Desjardins and his associates seek, while our advanced Protestants worship still at the feet of the apostles and of the church fathers. In answer to the objection against the preëminence given to action, the words of the Apostle Paul might be quoted: "And now abideth faith, hope and charity, these three. But the greatest of these is charity." Now charity is not faith, but action. But to appeal to a better authority, experience, we would remark that faith, not faith in creeds—that does not mean anything to the mass—but faith in the moral principles, is established by action and can be best developed by action, for it is engendered by feeling accompanying moral activity, positively or negatively exercised.

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Before saying a few words on Desjardins' position towards the Roman Catholic Church, let us embrace at a glance, for comparison's sake, the two movements led respectively by Lavisse and de Vogüé

Ernest Lavisse personifies the idea of the renovation of the national life through education. His God is the nation; his gospel is "love your country and prepare to serve it worthily." This is without doubt a large and noble sentiment, able, in a proud and generous people, to check the dissolving effects of a life without purpose. The sight of a nation shaking off its pessimism, its sensualism, its nihilism, at the thrill of a newly awakened patriotism, is one of the grandest spectacles that history presents. It is a striking illustration of the saving influence of altruism. But is not the peace of our civilization endangered by this stirring up of national pride? Does it not bring with it the hatred of the enemy and the thirst for revenge? Is patriotism the true support of the moral life? No, it is but a tonic or a palliative. The devotion to one's country cannot be sufficient to a complete healthy, human life. In moments of intense national rivalry, it will be a powerful lever. But what will happen after the victory or the defeat?—Just what happens, after the encounter, in the well-regulated life of the prize-fighter. Civilized society needs a deeper and truer base of existence, including in itself patriotism and all other virtues. The citizen's life must have a purpose distinct from and above the service of his country, a faith having its foundation in his deeper moral nature. M. Ernest Lavisse does not deal with the true problem.

We have seen that le Vicomte Melchior de Vogüé is a charming writer, a beautiful mystical soul, not in the least inclined to asceticism, but rather having an artistic weakness for the beautiful,

especially when it is allied to grandeur and power. To the insufficiency of science and democracy, he advocates the addition of the Christian principles of love and sacrifice, and courageously declares his belief that nothing durable can be done nor will be done for the salvation of society independently of the Roman Catholic Church. There is room for some reforms in the church, but it is evidently not the business of its dutiful sons to point out to an infallible Pope the defects which mar the grand old structure and make it untenable to so many good men. We must, in all religious affairs, await the good pleasure of His Holiness. It appears that M. de Vogüé lulls himself with the hope that the imperative needs for some form of religion, which now torment the young generation, will prevail upon them to throw above board the clearest and grandest acquisitions of the past—the independence of the reason and the liberty of conscience—and cause them to imprison themselves in that senile ecclesiastical machine.

As soon as M. de Vogüé had promulgated his fancies, the French youth raised a loud cry, a good deal like a sneer, against the man to whom they had listened with joy and thankfulness as long as he talked of love and sacrifice only. Traditional authority, ecclesiastical authority, papal infallibility, cannot suit the people who made the Revolution of 1789 ; no more can the orthodox doctrines of the church satisfy the youth, nourished with the science of the nineteenth century. The agitation was greatest among those who regard the religious revival with a suspicious eye, and this spring the opposition crystallized into a society calling itself "*La Ligue Démocratique des Ecoles.*" Its programme manifests a strong anti-religious sentiment. To apply in all questions the severe scientific methods only, and to take as sole rule of conduct the reason, to proscribe all mysticism and all religionism, are the main articles of its statutes. To this negative side is added a positive one: the study of the social questions. This open and constituted opposition is very regrettable, but it was to be foreseen by the readers of de Vogüé's articles. Soon after the foundation of the Ligue Democratique, a sort of great inaugural was held at the "Hôtel des Sociétés Savantes," at which M. Aulard, the distinguished occupant of the new chair of the History of the Revolution at the Sorbonne, was the orator. He availed himself of the occasion to assail vigorously with arguments and raillery the neo-mystic-religious dreams. To them he opposed the principles of the Revolution and the declarations of science, on which alone the future can be built. It is in vain that some gentlemen of leisure, having found Chateaubriand's inkstand, have set about resuscitating the faded glory of the "Génie du Christianisme." The unknown God, whom they say young men are seeking, is only too well known. It is the God who, for so many centuries, has fettered the reason. M. de Vogüé, who, although not named in the speech, was the special target of the orator, answered in a bright but unsatisfactory article.[1] The Quartier Latin did not remain a passive spectator of the discussion. French students are always near the point of ebullition. Immediately the old politico-religious passions sprang up and superseded the tone of quiet religious earnestness which had swept over a good part of the Quartier. Catholics and Liberals, neo-Christians and Radicals, who had remained ill-defined, drew into battle array. Manifestations were held by the different parties. On May the 17th, Professor Aulard was to lecture on the "Convention Nationale" of 1793, and

[1] *Revue des Deux Mondes*, May 1, 1893.

specially on the discussion, then held, relative to the article on religious liberty. The amphitheatre was crowded long before the appearance of the professor. As he enters, cries of Vive Aulard! Vive la Revolution! arise. He hardly begins when a group of Catholic students leave the hall ostentatiously. They are followed by a large number of Liberals. In the street, blows are brought to bear on the debate, until the police intervention makes an end of the affray. Catholics and Free-thinkers are again at sword's-edge in the university. MM. Aulard and de Vogüé, the champions of the old rationalism and of the old clericalism, must deeply regret this explosion of political passions caused by their utterances, whatever may be their convictions. The safest anticipation that can be made on the issue of this regrettable conflict is that M. de Vogüé's influence on the students will be limited to the small number of Catholics. The wise keep away from these extremes, and instead of placing themselves under the auspices of secular or ecclesiastical historical events, they remain in the sphere of the revelations of consciousness. Only there peace and good will towards all can prevail.

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M. Paul Desjardins, as differentiated from Lavisse, views the problem in its essential and universal aspect. Sensualism and egoism, these inseparable sisters, born of a diminution of life, itself the result of the lack of great, disinterested ambitions and of the negations of science, are the real evils. What is needed is a solid basis on which an individual—and consequently also a national—life can be built. Where is that foundation to be found? The exterior supports of right living have fallen down. Exterior revelation, tradition, reason itself are disbelieved. Desjardins seeks the answer to this question in the inner life of man, in the centre of his nature. When everything else has failed, it remains in us *a sense of direction;* there is the answer. Do not rely on an exterior revelation, do not trust in any external authority, nor even in reason, but rely on your deeper self. Learn from the facts of conscience, of which you are the passive witnesses. Do what they bid you do. Submit to your nature, to nothing else. Obey duty. The consequences of this obedience you will accept as necessary, as true. Faith in certain ideas will result from it, but it will be a rational and unshakable faith, for it will be born of your own experience.

There is surely nothing absolutely new in this solution, which we believe to be the purport of M. Desjardins' message, although he nowhere expresses it in that form; nevertheless, we see in it the formulation of a truth which has always been the ultimate guide of man's life, unconsciously at first and rising slowly to consciousness with the progressive evolution of humanity; man does not acknowledge anything binding except that which he finds in himself; he may project it outwardly; he may first perceive it in some other person, who, then, becomes a revelator, and he may submit to his authority; or he may find the expression of his religious needs in the articles of a creed, and, unconsciously reversing the psychic process which has taken place, attribute to the creed itself an authority which, in reality, it owes to its symbolic expression of soul-contents which have not yet reached self-consciousness. The essence of a person's belief rests always on facts directly experienced, whatever may be the person's opinion on church and on creedal authority. The principles on which Desjardins proceeds, ap-

pear to be the crowning achievement of a process, for which the very clumsy word "moral-self-consciousification" might be suggested. The open declaration of this principle and the effort put forth to make it triumph, constitutes, it seems to us, a religious revolution, a revolution corresponding in the moral and religious world to the social and political Revolution of 1789. Then men were declared politically free and equal; to-day they are declared religiously independent from all external authority. In 1789 the sovereign authority of reason came to the self-consciousness of the mass; to-day the sovereign authority of conscience arrives to the self-consciousness of the mass. To the declaration of the political and social rights of man, France is about to add the declaration of the religious and moral rights of man. The psychologists of all lands will watch with intense interest the beginning of this new era, for we need not draw here your attention to the consequences of this revolution. It brings with it the realization of the more or less definite desires of the best men of all the civilized nations with regard to religious reforms. It heralds the fall of a religious authority not resting on the conscience. It is the death of tradition, which keeps us enchained to an age long over-grown; the fall of the intellectual creeds, the belief in which was and is still made the condition of salvation; but above all it is the aurora of a new day in which Christian love shall reign, because men will seek their inspiration in the holy revelations of their conscience.

M. Paul Desjardins appears to us the apostle of this Revolution. He has not, we must acknowledge, seen his way clearly from the first. Although in his early publications he declared himself neutral as to the various religious faiths, it appeared to many that he was leaning towards a reformed Roman Catholicism. Certain persons, for instance, related that he had gone to Rome and had received complete approbation from the Pope, and that from that day his work was under the tutelage of the church. M. Desjardins, indeed, went to Rome, but it was for the ostensible motive of obtaining a dispensation desired by some scrupulous Catholics in order to feel at liberty to work in common with heretics. The dispensation was granted. A month ago he defined clearly and decisively his attitude towards the church in general. Allow me in concluding to set forth before you this attitude as expressed in the articles published in the *Journal des Débats*, under the title, *La Conversion de l'Eglise* and *La Vraie Eglise* (the true church).

In the bold articles on the conversion of the church (November 1892), M. Desjardins proposes, without acrimony or violence, but also without tergiversation, what he considers to be the necessary transformations which the Roman Catholic Church must undergo in order to regain control over the people. It is in one word the return to the spirit of Christ. "The conversion through which the church must pass is a conversion of the heart. It must become again a school of love and of liberty. The church must bring to the Republic the breath which gives long life to cities in giving to each citizen that which is the whole of life according to Christ and according to reason, namely, the spirit of peace in self-devotion to mankind, and of faith in salvation through sacrifice. It must spiritualize itself previously to spiritualizing the people." The church is instituted perpetually to repeat by its example and by its speech: sacrifice yourselves; love and you shall live—and not to direct states. "The social duty of the church holds, then in these three rules: (1) To minister to the humble who carry the great burden; (2) To speak to them and to other men by means of love

and of self-sacrifice; (3) To develop among them that spirit of love and of self-sacrifice."

The church must then break with ambition, with wealth, with power, with the spirit of domination and of coercion. The whole of its authority must consist in the natural ascendency exercised by respectability and amiability. It must no more be a wheel in the state machine. The spiritual must separate from the temporal. The shocking anti-Christian inequalities established by the traffic made with ceremonies and even by the dispensation of the sacraments must disappear. As no one can serve two masters, it is also evident that the church must detach itself from the worldly solemnities and customs which make of it a temple of Mammon, and that the false splendor of devotion, the various classes of marriages, etc., must be set aside.

The reader asks himself, what would remain of the Roman Catholic Church if all these reforms were realized? What would become of the hierarchy, of the entire submission of the one to the other? What of the infallible authority of the Pope? And as to dogmas, sacraments and other ordinances, a few lines at the end of the last article of November permit us to infer a radical denial of the intrinsic value given them by the church: "Public worship itself must not be overdone; inner fervor gives to it its whole value."

Under the title, *The Conversion of the Church*, M. Desjardins was, unwittingly or not, demanding the destruction of the Roman Catholic Church. This series of articles was suddenly interrupted, and the author had not yet clearly declared what everybody wanted to know, whether he entertained the intention of a future adhesion to the Church of Rome. At last, five months later, in April last, the *Journal des Débats* published the long expected conclusion. It was entitled *La Vraie Eglise.* In it M. Desjardins explains why, to remain Christian, he cannot become Romanist. The time elapsed between Christ's gospel and our century is to be regarded as a sort of prolongation of paganism still enveloping the pure Christian spirit. "Modern metaphysics has come to take up the work of Saint Paul and to consummate the rupture between the spirit and the law. Let us not hesitate, we must help on. Here is the hope of the future, not only of humanity, but also of true Christianity, which is but dawning."

Let us unite with Desjardins and his friends in the effort to shake off the encumbrances of the past. Let not the veneration we have for the works of the dead blind us to the needs of the living. The throes through which France is passing will soon reach to other nations. These throes are not to be feared, for they are but as the struggle of the butterfly endeavoring to set itself free from its gross-imprisoning chrysalis; man is about to take an epoch-making step toward the more complete realization of his divine nature.

PSYCHOLOGICAL LITERATURE.

Les Phénomènes Psychiques et la Température du Cerveau. ANGELO MOSSO. Phil. Trans., Vol. 183, pp. 299-309. 3 Figs. (Croonian Lecture, 1892.)

Mosso's previous work upon circulation in the brain, and also upon mental and muscular fatigue, have given him most valuable preparation for attacking the delicate problem of brain temperature in relation to psychic activity. His earlier work revealed the fact that blood pressure in the brain rises during mental activity, and also that there may be fluctuations of blood pressure, which are independent of psychical activity. Mosso had also become suspicious that Schiff's theory of brain temperature needed modification, and had been made duly alive to the fact that brain temperature would mean very little unless the temperature of the blood was taken at the same time.

Experiments were made upon animals under morphia and anæsthetics, and also upon man. Delicate mercurial thermometers, made expressly for the purpose by M. Baudin of Paris, and capable of reading to 0°.002 C., were employed, the bulb being generally placed between the dura and the skull, but in some cases under the dura. Temperatures of interior of body, blood in carotid artery and brain were taken simultaneously.

In general, brain temperature is lower than interior body temperature. To demonstrate that this is due to radiation of heat from surface of head, it is only necessary to keep the animal in a medium heated to the temperature of the rectum. Under these circumstances, the temperature of the brain is always higher than that of interior of body by from 0°.50 to 0°.63 C. This proves that the brain is the seat of active chemical changes, which make it a great thermogenic organ. Under ordinary conditions, great psychic activity or the action of exciting drugs may cause sufficient increase of heat production in the brain to raise its temperature as much as 0°.3 above that of the internal organs.

Curves showing superimposed, the internal, carotid and brain temperatures give great clearness and precision to the subject. In profound sleep under morphine, the curves of brain and carotid temperatures are seen to very nearly coincide,—the brain temperature a trifle lower, and both somewhat lower than the internal temperature. Shouting in the ears of the animal causes a slight rise of brain temperature, and electric stimulation of the cortex is shown to cause a sudden rise of temperature considerably above that of the carotid and almost up to internal temperature, the animal showing no signs of waking. Experiments made while the animal is awake show a great amount of heat production in the brain over that produced during sleep. This seems to be needed for the mere maintenance of consciousness. Greater psychic activity occasions a scarcely perceptible increase in temperature, the greatest rise recorded from this cause being 0°.01 C.

Experiments with various anæsthetics and narcotics show that these suspend the activities of nerve cells to such an extent that in deep anæsthesia, electrical stimulation produces no rise in brain temperature. Elective action of stimulating drugs on the different tissues is well shown by the injection of cocaine, ten centigrams of the hydrochlorate causing a rise in brain temperature of 0°.36 C., no change being observed in either muscles or rectum. The effect of cocaine upon the brain is rendered more conspicuous by combination with curare. In a deeply curarized dog, the temperature of the whole body was observed to rise 4°, from 37° to 41° C., within a half hour after the injection of the cocaine. That the effect upon the brain caused this rise is shown by the brain temperature being 0°.2 C. above that of the rectum during the time. The experiments are of special interest as indicating active chemical changes within the brain.

The Changes in the Optic Tracts and Chiasma in a Case of Unilateral Optic Atrophy. WILLIAMSON, R. T. AND M. R. C. P. (LOND.) Brain, Part LVIII. p. 230. 1892.

Hannah T., age 56. Complete loss of vision R. eye; atrophy of R. optic disc. Left eye and L. field of vision, normal. Sudden onset of blindness in R. eye after an attack of rheumatism, four years previous to death. Findings agree in the main with those, for similar cases, recorded by Purtschner and v. Gudden. The optic nerve of right side was much shrunken, and contained almost no healthy fibers. Left optic nerve was normal. In the chiasma, the degenerated optic nerve fibers were found to pass to the inferior surface of the opposite side. In the optic tracts, an area of degeneration could be plainly seen occupying the central area of the right side (uncrossed fibers). The left optic tract was much shrunken, and showed degeneration chiefly in the inner half of the inferior surface. Indications of degeneration extended also to the outer half of the inferior surface and to the outer surface. Hence uncrossed fibers occupy the central portion of the optic tract; while crossed fibers, with slight modification of Purtschner's statement, lie along the periphery of the tract. Microscopical examination was made by Weigert's method. A series of eleven well selected drawings add great clearness to the description.

I.—Il cervelletto; nuovi studi di fisiologia normale e pathologica. LUIGI, LUCIANI. Firenze, 1891.

II.—Sull' origine e decorso dei peduncoli cerebelleri e sui loro rapporti cogli altri centri nervosi. MARCHI, VITT. Firenze, 1891.

Of the above papers, the first deals with the physiology, the second with the anatomical connections of the cerebellum, as shown by degenerations resulting from partial or entire extirpation.

Dogs and monkeys were employed for the experiments. The cerebellum was removed, wholly or in part, under narcosis produced in dogs by hypodermic injection of morphia and chloral (morphia 2.5 centigram, chloral 1 gram), and in monkeys by morphia and chloroform.

The principal operations studied were: *Extirpation of the middle lobe of the cerebellum; extirpation of the whole cerebellum; extirpation of one-half of the cerebellum.* This latter was done by dividing the vermis in the median plane by means of a Græf's knife.

Operations upon the cerebellum at best are difficult, great care being necessary to avoid excessive hemorrhage and the injury to

adjacent parts of the brain, and especially of the medulla. Hemorrhage was arrested by means of bits of sponge soaked in corrosive sublimate solution (1-1,000). The operations were eminently successful, as is attested by the fact that Luciani kept his animals months and even years after total extirpation of the cerebellum. In this way, he was able to distinguish between temporary and permanent effects.

For convenience, the results are classified under five categories: 1. *Phenomena of irritation*, disturbances of innervation attending more or less closely upon the operation. 2. *Phenomena of deficiency.* These are to be attributed to lack of the portion of the cerebellum, which has been removed, and form, perhaps, the chief feature of interest in the research. The following symptoms are noted as characteristic of cerebellar deficiency: Lack of voluntary power—*asthenia;* lack of muscular tone—*atonia;* and uncertainty of movement, tremor, oscillation—*astasia.* It is impossible to separate these clearly from the next group. 3. *Phenomena of compensation;* classified as organic,—a gradual lessening of the phenomena of deficiency and functional,—abnormal movements directed to correct faulty movements occasioned by deficiency. 4. *Phenomena of degeneration;* in other parts of the nervous system following cerebellar extirpation. 5. *Trophic phenomena;* it is claimed indirectly, that the cerebellum exerts an influence upon the nutrition of the tissues.

I.—Effect of division in the median plane. In the case of three animals, the cerebellum was divided in the median plane. One dog lived twenty-two months after the operation, and was carefully studied. Some difficulty in co-ordination was noted at first, and, as a phenomenon of deficiency, lack of energy in the performance of voluntary acts and diminution of muscular tone were the more lasting results. Tracings of the dog's footprints were taken by dipping the feet in different colored inks; and these showed that *compensation* became so perfect that it was difficult to discern anything abnormal in the gait. The general disturbances are, however, sufficiently marked to lead the author to conclude that the cerebellum is physiologically a unit as well as anatomically, and not divisible bilaterally without interfering with its function.

II.—Effects of removing the middle lobe. Four dogs and two monkeys were operated on. The case of one monkey may suffice to illustrate the other experiments of this class. Immediately after the operation, there was noted tonic contraction of neck muscles and tonic flexion of upper and lower limbs. By the next day, the animal was able to extend a weak tremulous hand for fruit, but if placed on the floor, could not keep its feet, and tended to fall from right to left. Second day after operation, the monkey lay with limbs flexed, and neck and trunk in continual oscillation. After about ten days, it had regained power to walk slowly and with fair co-ordination.

By raising a monkey from the floor by means of a belt around the waist and a pulley, allowing it to hold on with its hands to a dynamometer ring fastened to the floor, a method was devised of testing voluntary power or muscular strength. In this way, a healthy monkey would pull 8 to 10 kilos. After removal of vermis, he was able to register only 3 kilos. In a month, however, he was able to pull 5.5 kilos. In two months, recovery had become so complete that it was distinguished from the healthy monkeys with difficulty. In a year, muscular power had risen to 9 kilos. The dogs gave similar results, except that instead of flexion, the limbs were extended.

III.—Effects of incomplete unilateral extirpation. (Extirpation of one lateral lobe.) This operation was performed upon dogs. The symptoms were in the main similar to those attending extirpation of the vermis, but differed in some points. The *irritative phenomena* consisted of curvation of the spine toward the wounded side, with tonic extension of the anterior limbs on the same side, and rotation of the body upon its longitudinal axis from the wounded toward the healthy side with strabismus in the same direction. *The phenomena of deficiency* were confined to the side of the lesion. *Compensation* took place gradually, so that the animals regained power to walk.

IV.—Effect of removal of one-half of the cerebellum. (Both dogs and monkeys.) Monkey, right half of cerebellum removed June 26, 1884. After operation, the head was bent to the right, and rotated toward the left, rotation about long axis of body from right to left.

June 27.—Still a tendency to rotate from right to left, which the animal tries to avoid by catching hold of something with the left hand. The head is held as before. Both limbs of the right (operated) side are held flexed. The right pupil is more contracted than the left, with strabismus of the left eye inward and upward, and nystagmus of both eyes.

June 28.—Pupils of more equal size, tendency to rotation less. In attempting to take food, there is great oscillation of right arm.

June 29.—The above phenomena present, but less marked. The right leg has relaxed; tonic flexion of right arm persists.

July 1.—Phenomena of irritation and deficiency decreasing steadily. The animal takes fruit with the left hand. It uses the limbs of the left side, while those of the right hang apparently inert.

Middle of July.—Begins to use right arm more freely. Strength of arms as registered by dynamometer, 3.5 kilos.

September.—Traction of arms, 8.2 kilos. Great improvement in walking, though it raises its feet high off the ground. Head is still inclined toward the right, and slight strabismus persists.

April, 1885.—Still timorous and suspicious. Head is inclined to the right. In walking, the body is bent toward the right, and progression is in consequence obliquely forward and to the right. Indecision of voluntary movements is noticeable. Food is invariably taken with the left hand. There is still slight strabismus, the right eye turning inward, the left outward and upward. Slight ptosis of the right upper lid is also noted.

The phenomena of irritation and deficiency are what would be expected. The fact of special interest is that compensation does not become perfect. One-half of the cerebellum cannot perform the functions of the other half; and the cerebral hemispheres, with one-half of the cerebellum present, are not able, at least in ten months' time, to render movements and position bilaterally symmetrical.

One lateral lobe was removed in case of dogs with the vermis destroyed, with results in the main like those just enumerated for *hemi-extirpation.*

IV.—Effects of complete extirpation of the whole cerebellum. Operation on both dogs and monkeys.

June 12, 1886.—Entire cerebellum removed from a monkey. After operation, tonic flexion of both arms, the right more than the left; was noted also slight convergent strabismus of both eyes. It is unable to maintain an upright position. In about an hour, the animal was able to crowd its back into the corner of its box, thereby holding itself upright, and to take a piece of fruit with its left hand.

June 13.—Still keeps its back in the corner of its box, and holds out both hands when offered food. Deprived of support, it falls in a lump, and is unable to raise itself up again.

June 14.—Condition similar to that of previous day. When unsupported, tends to rotate from right to left. It can climb up the side of its cage, but cannot remain there without support.

June 16.—Placed on the floor, unsupported, it takes a few steps forward and soon falls toward the right. In extending its hand for food, there is uncertainty of motion not previously noted.

July.—When sitting, head and trunk oscillate, and eating is accompanied with constant tremor of arms and trunk. It walks more slowly than a normal monkey, in a hesitating, oscillatory manner and zigzag course. Tendency to fall is less marked.

August.—Ataxic phenomena about the same as in July.

October, 1886—June, 1887.—No change; possibly a slight improvement in health.

July, 1887—January, 1888.—Ataxic phenomena persistent. It walks with limbs far apart and continual oscillations of head, and descends a ladder with great care and deliberation. During this month, the animal was killed. Autopsy showed that the entire cerebellum had been removed, except a small portion of each flocculus. The cerebrum appeared normal.

For complete extirpation of the cerebellum, the phenomena of irritation and deficiency resemble those found in extirpation of the middle lobe, except that they are more intense, last longer, and are more widely diffused.

VI.—Effects of extirpation of the cerebellum, combined with uni- or bilateral destruction of the sygmoid gyrus.

Compensatory movements were naturally supposed to arise from the motor regions of the cerebrum; and to test this point, the experiments upon the sygmoid gyrus were undertaken. Only dogs were employed.

One-half of the cerebellum was first removed, with results already described. The second operation consisted in partial destruction of both sygmoid gyri. This caused paresis of the extremities and defective sensibility. Finally, the remaining half of the cerebellum was removed. The phenomena of deficiency are now found to be persistent. Eleven months after the final operation, the dog was unable to walk without support. The experiments bear out the conclusion that deficiency of cerebellar innervation is compensated for by motor-sensory areas of the cerebral cortex, especially that part located in the region of the sygmoid gyrus.

A long discussion follows, explanatory of the above observed phenomena. Here the chief interest centres about the phenomena of deficiency, and these lead the author directly to his view of cerebellar function.

In the first place, Luciani's experiments show that cerebellar deficiency is manifested only in the sphere of voluntary movement. Sensation, instinct or intelligence is not affected. All the special senses, dermal and muscular sense, is intact. The instincts of self-preservation and reproduction are as active as ever. There is noticeable, however, in dogs, a certain listlessness and lack of energy. Aside from this, nothing of a psychic nature is to be correlated with the function of the cerebellum.

The cerebellum is, therefore, not an organ intercalated in the main paths of the cerebro-spinal system, but a tonic reinforcing centre placed alongside the main paths. It is more and more

highly developed as we ascend the vertebrate series, but in no form does complete muscular paralysis follow extirpation of the cerebellum. Further, the power to co-ordinate muscles remains. The disturbance of co-ordination so often noted in cases of cerebellar lesion, is not primarily lack of co-ordination, but lack of the tone, the energy to hold the muscles in a co-ordinated contraction. So that when the animal by careful attention has co-ordinated his muscles to maintain a certain position, if his attention is called away to something else, food, for example, he suddenly falls.

It remains to consider what Luciani calls the *trophic phenomena* associated with lesions of the cerebellum. Degenerations in the various parts of the brain and spinal cord plainly show that the cells of the cerebellum are the trophic centres for nerve fibers, which pass beyond its limits. These will be described later. Further, for the first few days after operation in some of the animals, polyuria, glycosuria and acetonuria were present. Marasmus, without apparent cause, alopecia, erythema and eczema, conjunctivitis and keratitis occurred in several cases. It is difficult to see in what way these prove any direct trophic action. There is little uniformity in their occurrence, and, moreover, they all heal readily upon the application of antiseptics. Fatty degeneration, with increase of muscle nuclei in the muscles of the limbs, is also noted as a trophic phenomenon.

A lengthy discussion of all the theories concerning the functions of the cerebellum follows, and the book closes with a chapter upon "first lines of a new doctrine." This has already been hinted at, and may be made plain with a few words in addition. The cerebellum is a nervous system by itself, added to the main system for the "sthenic," "tonic" and "static" reinforcement of motor impulses. Its action upon the body muscles is mainly direct, thereby differing from that of the cerebrum, whose action is crossed. Experiments do not show that the vermis is of any greater or any different functional value from the hemispheres. In fact, the organ is not a collection of centres or parts, which exert a special influence upon special muscles or groups of muscles, but it is a physiological unit. From it flows continuously and quietly a stream of nervous impulses to the whole muscular system. Removal of the cerebellum is thus shown in lack of muscular tone, and not in paralysis, partial or complete, which is apt to follow extirpation of portions of the cerebrum. The cerebellum is a reinforcing organ for the cerebro-spinal system.

The brains and spinal cords of the dogs and monkeys operated upon were given to Marchi for the purpose of working out the degenerations resulting from the partial or complete extirpations. Marchi employed Weigert's hæmatoxylin method, and a method of his own, which consists of hardening the specimens in Müller's fluid for a short time, and then further treating small pieces, 1 cm. cube with a solution of Müller's fluid and osmic acid (Müller's fluid, 2 parts; osmic acid, 1 per cent., 1 part), for 8 to 10 days longer. Degenerated portions by this process are stained black.

Most instructive are naturally the results following extirpation of half the cerebellum. These results for extirpation of right half are briefly as follows:

(a) In the superior peduncle and in region of the cerebrum, the method of Marchi gave evidence of degeneration in both peduncles, in the right more than in the left. There was much degeneration of the left red nucleus and a little of the right. There were also

evidences of the degeneration in the fillet, in the third pair of nerves, in the pyramidal tracts of the crura, in the posterior longitudinal bundle, and in the right optic tract.

(b) Complete sclerosis of the right middle peduncle was found, and the degeneration extended over the middle line, and involved the gray matter of the pons. Degenerated nerve fibers were also found in the right fifth nerve, in the fillet, in the posterior longitudinal bundle, and in a small bundle of fibers which lie behind and external to the superior peduncles.

(c) In the region of the inferior peduncles, the median portion of the peduncle and the external portion of the restiform body of the same side were degenerated. The other parts involved are certain of the striæ acousticæ and a portion of the external auditory nucleus, many fibers of the ascending root of the fifth nerve, the fillet, the interolivary layer, the posterior longitudinal bundle, some fibers of the hypoglossal nerve and of the pyramids. There also appeared to be some degeneration of the olivary body of the opposite side. The above degenerations were shown by Marchi's method. Wiegert's method did not give quite so diffuse showings.

(d) In the spinal cord degeneration was demonstrated by Marchi in the antero-lateral region of the same side with the extirpation, the affected fibers lying in part in the anterior portion of the direct cerebellar tract, and in part in the anterior pyramidal tract.

Due to extirpation of the vermis, Marchi demonstrates slight degeneration of the superior peduncles, although in the fillet, the posterior longitudinal bundle, the roots of the third nerve and the optic tracts, degeneration was considerable. This would indicate that the superior peduncles arise mainly from the lateral lobes. In the middle peduncles, all transverse fibers were degenerated. The inferior peduncles showed degeneration only in the lateral part of the restiform body. Beside the above, degeneration was demonstrated among the fibers of the trapezoid body, and in the roots of the fifth, eighth and twelfth cranial nerves, and also in the antero-lateral columns of the spinal cord.

Marchi summarizes his results as follows:—

1. The decussation of the superior peduncles is not complete. A small bundle of its fibers goes to the optic thalamus of the same side, while the main part of the peduncle ends in the red nucleus of the opposite side.

2. The middle cerebellar peduncles are not simply commisures between the lateral hemispheres. Many of their fibers end in the gray matter among their pyramidal bundles of the same and of the opposite side.

3. The inferior peduncle sends a bundle of fibers to the olivary body of the opposite side. It probably consists of afferent and efferent fibers.

4. The posterior longitudinal bundles and the fillet arise from a common source in the vermis. They pass down with the middle peduncles, and become connected with the nuclei of the cranial nerves, the nuclei of the pons, the corpora quadrigemina, and probably with the corpus striatum. At the level of the olive, the posterior longitudinal bundles unite with the fillet and thus make a connection between the antero-lateral regions and the anterior horns of the spinal cord.

5. The cranial nerves are intimately connected with the cerebellum by means of the fillet and posterior longitudinal bundles.

6. The origin of the three peduncles is diffused over the cerebellum, but the middle lobe gives rise to most of the fibers of the middle

peduncles and the nucleus dentatus, to the greater part of the fibers of the superior peduncles.

The above shows a more intricate connection of the cerebellum with other parts of the nervous system than has hitherto been demonstrated.

Sezione mediana antero-posterioe del verme del cervelletto. GALLERANI, G., AND BORGHERINI, A. Revista Sperimentale di Freniatria e di Med. Legale, Vol. XVIII. p. 369-388. 1892.

The authors state at the outset that their work was compiled before Luciani's book, Il Cervelletto, appeared, and that they will not take this occasion to discuss it. The work is further a continuation of experiments reported in the same journal in 1888. They bring forward but two experiments, both upon dogs, in the first of which the median division of the vermis was partial, extending about two-thirds its depth; the second, it was complete.

The first dog, one day after the operation, was unable to stand upon his feet. On the second day, he made weak attempts at walking. The trunk oscillated. On the fourth day, walking was still performed with legs half flexed. The gait was plainly ataxic, and ataxy of head and neck was seen when the animal tried to take food into his mouth. This condition of things is still present upon the ninth day. Observations upon the thirty-third day show that there is still ataxy, and lack of power to co-ordinate the muscles properly. This is seen especially when the animal begins a certain action. Once started, he can go on fairly well. He can run well, but in a slow walk his course is zigzag. All symptoms have about disappeared by the ninety-fifth day, when the dog is killed. Autopsy shows that the incision extended through about two-thirds of the depth of the vermis, and was healed with connective tissue.

In the case of the second dog, the phenomena are more marked and persistent. Upon the 142d day, the erect posture was maintained with oscillation of the trunk and with legs wide apart. Voluntary acts are done with slowness and attention, and although considerable improvement has been made in this respect, they are still ataxic. At this time, the animal was killed, and it was found that the division of the vermis was complete and remained so, the wound having become filled with connective tissue.

The authors consume three pages with their conclusions from these two experiments. Their aim in this seems to be to refute the idea of Schiff to the effect that the asymmetry of cerebellar lesions is of special importance in determining the amount of disturbance, and to contradict everything possible in Luciani's book. They further insist upon the correctness in the main of the old view, viz., that the cerebellum stands in close relation to the co-ordination of voluntary movements, both such as are directly voluntary and such as have become automatic by long use. The action of the cerebellum, they would explain, as Wundt does, as a kind of complicated reflex, which is composed on the one side of all the centripetal impulses from the skin, muscles, and organs of special sense, and upon the other side, of all the motor impulses which keep the body in equilibrium or render movement orderly. A lesion of the cerebellum will create a disturbance of co-ordination, not from the fact that it is asymmetrical, which has no influence in itself, but in proportion as it severs connection in the cerebellum with the different parts of its own mass, and especially as it interferes with the normal connections of the cerebellum with the other parts of the nervous system. The vermis connects the lateral hemispheres,

and hence it is that lesion of the vermis is more apt to produce motor disturbance than lesion of the hemispheres.

The above summarizes the points of chief interest in their paper. It is followed in the journal by a six-page "nota critica" by Luciani, in which he is not careful to spare the feelings of the "youthful authors," as he repeatedly calls them. That it is difficult to make out the exact meaning of a number of Gallerani's and Borgherini's concluding statements may be seen from the fact that Luciani himself, presumably a master of the Italian language, is unable to do so. Before some of their sentences, Luciani says that he "stands with open mouth, like the country bumpkin before his curate, to whose long words his intelligence does not reach." He is not slow, however, in asking whether in this case the intelligence of the curate or the audience is at fault. The criticism abounds toward the end in such expressions as "I giovani autori" "Qui l'audacia dei valorosi giovani;" "Questo concetto dottrinali;" and the like.

In as far as this is a family quarrel among Italian physiologists, we do not wish to follow it. But if there is anything to be said in favor of the old view of cerebellar function, now is the time to say it. If after the entire cerebellum had been oblated, as in the case of Luciani's monkey, the animal is able, within an hour after the operation, to "reach out a trembling hand for fruit," it would seem to be proof positive that the mechanism for muscular co-ordination must be somewhere else than in the cerebellum. If on the other hand a lesion of the cerebellum can be made which causes "oscillation," "ataxy," and in general faulty co-ordination of the muscles one hundred and forty-two days after the operation, we are glad to have attention called to the fact.

The Origin of the Sertoli's Cell. WATASE. Am. Naturalist, Vol. XXVI. May, 1892, p. 442.
On the Significance of Spermatogensis. Ibid. July, 1892, p. 624.
On the Phenomena of Sex Differentiation. Ibid. Jour. of Morphology, Vol. VI. p. 841.

In the above papers Watase advances some new experiments which disprove, or profoundly modify, the old dictum of the cytologists, "Chromatin is unsexed." By the use of differential stains, at any rate, male and female nuclei are found to react differently. Watase employed the three aniline colors, viz., cyanine (blue), erothosine and chromotrop (red), and found that the nucleus of the ovum stained *red*, as in the case of most tissue cells, while that of the spermatozoan stained a *deep blue*. This fact Watase has succeeded in demonstrating for a long and widely different series of animals, including both invertebrates and vertebrates from the starfish to man.

The experiments are chiefly confirmatory of Auerbach's recent investigations, but at one point at least they constitute a decided advance. This is the fact given by the author as the reason for the last paper on the list, that, while the male and female nuclei as found in ovum and spermatozoan are as unlike as blue and red, after penetration of the spermatozoan and before the union of the two, the male and female pro-nuclei come to stain exactly alike.

We are led by the author's statement to the effect that the above papers are of the nature of preliminary communications to look forward to a more complete account of the work. The above is sufficient at any rate to again emphasize the folly of setting bounds to what may be accomplished by the proper refinement of method.

Sur un procédé destiné à évoguer les images motrices graphiques, etc. CHARCOT (J. B.). Progrés Médicale, 18 Juin, 1892, p. 478.
Sur un nouvel Appareil destiné à l'étude experimentale des Sensations kinesthésiques. JANET (PIERRE). Revue Philosophique, Nov. 1892, p. 506.

Charcot describes an apparatus devised by him to secure kinæsthetic writing sensations in a patient—free from contributions, from touch, pressure, sight, etc. The apparatus consists in a writing-pencil, long enough to be held by two hands, one that of the patient and the other that of the experimenter. The experimenter writes with one end of the pencil beneath a platform, and thus carries with the pencil the hand of the patient (above the platform), whose movements of finger, hand, etc., are made to reproduce his own by a combination of balances. He studies with this instrument cases of verbal blindness, in which kinæsthetic writing sensations remained intact, such patients understanding words only by tracing them. He thus establishes the reality of the phenomena of word-perception by kinæsthetic sensations (cf. cases of Sommer, J. M. Charcot, Pick, etc.), and concludes that there is a functionally distinct motor graphic center.

Janet points out the importance and convenience of the apparatus of Charcot and reports having successfully used it in demonstrating the now well-known unconscious writing movements by the kinæsthetic hands of hysterical patients.

J. M. B.

Die Entstehung und Ausbildung des Muskelgewebes, insbesondere der Querstreifung desselben als Wirkung der Thätigkeit betrachtet: EIMER. *Zeitschrift für wissensch. Zoölogie*, LIII. Suppl. 67.

A detailed plea for a "physiological" conception of the development of muscle-tissue. "The morphological property is the result of functional activity." In single-celled creatures, contractile substance arises gradually out of the protoplasm. Many comparative and embryological facts are stated in the course of a survey of the animal series in support of this general view of the rise and of the striation of muscle. Working backward from the medusa, in which the striation is clear, he finds "all the stages between such definite striation and its complete disappearance." Among his interesting cases are the "breast-muscles" of flies, which he supposes to lose during the season of rest (winter) the striation gained during the flying season (summer). There are accordingly no original morphological divisions in muscle. Contraction waves leave markings which account for both the muscle fibres and the striation.

On the Perception of Small Differences, with special reference to the Extent, Force and Time of Movement. FULLERTON AND CATTELL. Philosophical Series, No. 2, University of Penn. Press, Philadelphia, 1892.
The Psycho-physics of Movement. CATTELL AND FULLERTON. Mind, N. S. I. 1892. 447.

The outcome of the valuable monograph of the authors' (first title above) with the results of experiments on the extent, force and time of movements are given in résumé by the authors themselves in the paper in *Mind* (second title above) which is in everybody's hands. We may refer the reader, therefore, to that

article for an authoritative, condensed statement of a conclusion adverse to Weber's Law, of another principle which one of the authors would substitute for Weber's Law, and of the grounds on which both these claims rest.

J. M. B.

Ueber Sensomobilität. SIGM. EXNER. *Pflüger's Archiv.* 1891, XLVIII. 592.

Following experiments (already published) of the author and Herr Pineles of Vienna, on the motor effects of sensory lesions, this paper discusses the various ways in which motor impulses are regulated or controlled by the sensations to which they give rise. The author finds three not sharply distinguished cases: 1. In reflex actions—where neither the original stimulus nor the sensation caused by the motor impulse reaches consciousness (*e. g.*, intestinal movements), or the sensation may affect consciousness (*e. g.*, contraction of the pupils) and be controlled by the will (*e. g.*, winking). This he calls *subcortical control* (subcorticale Regulirung). 2. This subcortical action is not limited to reflexes, but may control acts which are pre-determined and are to be set in operation by some stimulus and guided by attention (*e. g.*, focusing the eyes). This is *control by determination* (Intentionsregulirung). 3. A *conscious* movement calls forth sensations which are essential to the correct execution of the movement (as in speech). This is *cortical control.* Bodily movements are in a high degree dependent on the senses. Disturbances of sensibility give rise to motor disturbances by removing one or other of the above-mentioned kinds of control.

Berlin. H. C. WARREN.

Des Phénomènes de Synopsie. Par TH. FLOURNOY. Paris, Alcan, 1893.

M. Flournoy includes all the phenomena of "Colored Hearing" and of "Mental Forms" under the convenient and adequate name *Synæsthesia*—in place of which, to be sure, he himself usually employs the less defensible term *Synopsie.* One of the prominent features of the book is in fact the clearness and the usefulness of terminology and of classification, an especially important merit at this time, when the reaction against the formalism of classification without observation has resulted in the opposite tendency to make of psychological records a bare, formless diary of facts. The phenomena of synæsthesia are divided into three main groups: "*photisms*," among which are included, as by Bleuler and Lehmann, all the varieties of pseudo-chromesthesia; "*Schemes*," comprising not only "forms" *(diagrammes)* associated with series of words or numbers, but "symbols," or particular figures associated with single letters, numerals, colors and the like; and "personifications," in which the associated factor is no mere color or form, but has become richer and more concrete. From the standpoint of intensity, the phenomena are "objectified," "simply imagined," "localized" or "thought;" M. Flournoy has never observed a case in which color or form is actually objectified, but admits the possibility, chiefly on the testimony of Herr Ed. Gruber. The book is the result of the detailed observation by M. Flournoy of particular cases and of a statistical investigation undertaken by M. Claparède, in which 694 answers were received to 2600 circulars of inquiry. Not the least diverting part of the book, especially to any one who

has ever undertaken statistical study, is the humorous comment upon the indifference of the public—even of a university public—to psychological inquiry. One-half of the 694 answers recorded positive facts of synæsthesia; but M. Flournoy properly concludes that most of the 1900 who failed to answer would have replied in the negative. Of the 371 positive cases, more than one-half include photisms (a conclusion entirely at variance with the results of my own observation of more than 200 cases). Forms are described in detail under the heads "*forme-matière-localisation des diagrammes*," and the text is illustrated by more than 100 reproductions of forms. The careful tabulation of the colors assigned in 943 different cases to single letters is compared with the similar formulations of Fechner and of Bleuler and Lehmann. The result is the demonstration of the apparently complete individuality and lawlessness of such identifications of colors with letters. Flournoy detects a "Loi de clarté," in accordance with which *i* and *e* are usually light (or bright); *a* and o usually of medium intensity; and *u* and *ou* dark;[1] but even this is contradicted by the results of President Jordan's observation and of the Wellesley statistics, which record *o* as commonly white or light.

Less than one-ninth (46) of M. Flournoy's subjects connect colors with consonants, while two-thirds (247) have colors with vowels. This result contradicts my own, but is founded on the study of a far larger number of cases, and accords with Galton's results. It is noticeable, however, that the form of inquiry which included consonants, diphthongs, words, music, etc., in one question, facilitated a carelessness of response at just this point.

M. Claparède's questions were indeed too simple and too condensed to permit statistical justification for many of M. Flournoy's conclusions. These are all, however, based on observation and are uniformly well-considered and undogmatic. The narrowly "physiological" theory of synæsthesia is opposed and, with a passing mention of the influence of the habitual, and of vivid, intellectual association, the chief explanation is found in an emotional association, while emotions are defined in terms of Dr. James' theory as "*sensations de retour, dues aux modifications réflexes produites dans toute l'étendue de l'organisme par* [*une*] *perception.*"

MARY WHITON CALKINS.

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[1] The reference of course is to the letters as pronounced in French.

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